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PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*On Temporary Deligation of the Abdominal Aorta.* By WILLIAM STOKES, Jun., Surgeon to the Richmond Surgical Hospital; Lecturer on Theoretical and Operative Surgery Carmichael School of Medicine; Fellow of the Royal Medico-Chirurgical Society of London, &c.

AMONG the conditions which militate strongly against successful results being obtained in treating aneurismal tumours by arterial deligation, two prime causes for the want of success, which so frequently attends such operative procedures, may be mentioned. These are—first, the late period at which the operation is generally performed, after the development of the aneurism; and, secondly, the hemorrhage which so frequently results from ulceration of the vessel, caused by the continued pressure of the ligature. Sir Astley Cooper was quite aware of the first of these causes of failure, and in the record of the case in which he for the first time deligated the abdominal aorta, regrets that he did not perform the operation at an earlier stage of his disease, believing that had he done so, the result would have been very different from what it was. As regards

the second cause of failure—namely, hemorrhage, it is unnecessary to name any authority in proof of it, as it is one which is familiar to every practical operating surgeon.

Ever since the case of subclavian aneurism, which was treated in the Meath Hospital by direct pressure on the arteria innominata by Mr. Porter, an operation which I had the honour of assisting at, and an exhaustive report of which from his pen, is to be found in the pages of this journal for November, 1867, the subject of the treatment of aneurisms by this, or some similar method as that adopted in the case I have alluded to, has engaged my anxious consideration. The substitution of some form of acupressure for the ligature, appears to me to be a means of possibly obviating the second of the causes of failure I have alluded to, the object of the acupressure being, not a permanent, but a temporary closure of the vessel; the occlusion being maintained, until such time as the tumour might become consolidated. Whatever may be the defects of acupressure, it cannot be denied, even by its greatest opponents, that its proper application is rarely, if ever, attended with any ulcerative process in the vessel on which it is applied. I determined, therefore, when the first suitable case presented itself, to give a modified acupressure or temporary arterial deligation, a trial in the treatment of aneurism, as the results of the treatment of so many cases of this disease by the Dublin method of compression, have shown, that often such tumours become consolidated, when the pressure has only been maintained for a very few hours.

During the past year, my friend and former colleague in the Meath Hospital, Mr. Porter, has described an instrument which, simple but ingenious in its construction, appears to me to supply all the necessary requirements for the treatment of aneurism by temporary deligation of the vessel on the cardiac side of the tumour. It is easy of application, can be removed without difficulty, and arteries of the largest calibre can, as the case I am about to record unquestionably proves, be efficiently occluded without permanently damaging any of the structures of the vessel, as the ordinary ligature invariably does.

We may now proceed to give, as briefly as possible, the particulars of the case of James Shiels, who suffered from an enormous ilio-femoral aneurism, and for the attempted cure of which, I occluded the abdominal aorta immediately above its bifurcation.

James Shiels, aged fifty years, was admitted into the Richmond

Hospital under my care on the 4th of last March, having been recommended to me by my friend, Dr. John Kirkpatrick. His occupation was that of a messenger or carrier for one of the commercial establishments of this city, and from the nature of his occupation he was obliged frequently to carry not only very large, but very heavy weights. He was always, according to his own account, a man of temperate habits, and never suffered from any rheumatic or other constitutional disease. About three months previous to his admission into hospital he observed, for the first time, a tumour in the right groin, which, however, did not give him any inconvenience or annoyance, or oblige him to abandon his usual avocation at that time. About two months afterwards, it became so very large and painful that he was obliged to seek for medical advice, and for some time he was under the care of Dr. J. Kirkpatrick, in the hospital of the North Dublin Union, and from that he was transferred to the Richmond Hospital. The diagnosis of diffused ilio-femoral aneurism was not attended with any difficulty. There was a large pulsating tumour occupying the right iliac and upper portion of the femoral region, extending downwards under Poupart's ligament, and upwards within an inch of the umbilicus. It reached as far as the linea alba in front, and past the linea semilunaris behind. It was soft and elastic to the touch, and when the hand was pressed over it a pulsation and fremitus were distinctly felt. On applying the stethoscope over the tumour, a loud bruit was heard; the pulsation in the right femoral artery could hardly be felt; there was no other symptom of any organic disease. There was no cardiac murmur, the only thing abnormal about the heart's action being an occasional intermission. The patient's face had a haggard, care-worn, anxious expression. He did not complain of pain when the knee was flexed and the thigh slightly drawn up, but any attempt to extend it was attended with extreme distress. His appetite was bad, and being very restless at night, required powerful anodynes in order to induce sleep.

The idea of treating this case by pressure, was the first I entertained, but the patient could not bear any pressure either above, below, or on the tumour. In any situation pressure was attended with such extreme pain that it had at once to be desisted from. The other modes of treating aneurism recently suggested were also inadmissible, as, for example, the injection of different fluids with the object of producing coagulation of the blood, the introduction of wire, manipulation, &c. The great size of the

tumour prevented us having recourse to any of these comparatively novel procedures, the efficacy of which, even in small aneurisms, is, in my opinion, very questionable. Two courses, therefore, remained. One was to abandon the idea of any surgical interference, which would have been to leave the patient to perhaps a slow, but certain and painful death; and the other was to attempt to bring about consolidation of the tumour by occluding the vessel above the seat of the aneurism. After much consideration I determined to have recourse to the latter of these procedures, and, if possible, to apply either a permanent or a temporary ligature on the common iliac artery on that side, or, if I found that diseased, to go higher and occlude the abdominal aorta.

Accordingly, on the 8th of March, I proceeded to expose the artery, and in doing so was ably assisted by my friends Mr. Hamilton, Mr. Porter, Mr. Henry Curran, and my clinical assistants, Mr. Alexander Montgomery, Mr. W. Browne, and Mr. Agmon Vesey; many eminent surgeons, also, honoured me with their presence, among whom may be mentioned Mr. Tufnell, Mr. Croly, Mr. Kirkpatrick, Mr. R. St. John Mayne, and others. The patient having been brought well under the influence of chloroform, I commenced by making an incision on the left side, semi-lunar in shape, five inches and a-half in length, extending from one finger's breadth below the tenth rib, to a point two finger's breadth above the junction of the outer with the middle third of Poupart's ligament. The incision was, in fact, the segment of a circle, the centre of which was the umbilicus, and the radius about two and a-half inches in length. A careful dissection was then made down to the transversalis fascia, and a small opening having been made in that structure, Ferguson's broad steel director was passed with great care between it and the peritoneum. There was considerable difficulty in separating these two structures from one another. In truth, their separation constituted the chief difficulty in the operation. Finding that the incision was not sufficiently extensive, I enlarged it upwards and inwards, dividing the structures on the broad director. In doing so a slight wound was accidentally made in the parietal layer of the peritoneum, and through this there was a small intestinal protrusion. In order to secure the vessel behind the peritoneum, I passed my hand into the pelvic cavity, separating with great care that membrane from its cellular attachments, and then tried to get a view of the great vessels by pushing the peritoneum and intestines,

which, unfortunately, were greatly distended, towards the right side. It was, in truth, attended with such extreme difficulty, that at one moment I feared I should have to abandon my project. However, after getting the patient well over on the right side, and carefully retracting all the tissues in this direction by means of a pair of very broad brass retractors, which Mr. Croly kindly lent me for this operation, I was enabled at last to get a good view of the abdominal aorta and its bifurcation. The view for the first time of this great vessel pulsating strongly, of the two common iliacs, and the commencement of the enormous aneurismal tumour, to a great extent overlapping, and adherent to the right common iliac artery, made an impression on my mind that time can never efface!

Owing to the way in which the tumour overlapped the iliac artery, and the adhesions which had formed between the aneurism and the vessel, I found that any attempt to deligate the common iliac would be impracticable. Accordingly, I passed Lüer's aneurism needle round the aorta just before its bifurcation, and attached to the ligature a piece of silver wire, which was then drawn round the aorta. The ends of this wire were then passed through Mr. Porter's artery compressor, and traction was made on them until all pulsation and bruit had perfectly ceased in the tumour. The ends of the wire were then secured to the ring of the clamp, and the tissues, &c., being replaced, the edges of the wound were brought together by numerous points of interrupted silver wire suture. Strips of adhesive plaster, compresses, and flannel bandages were then applied; both legs were bandaged from the toes upwards with flannel, and the patient then removed to bed. The operation was almost bloodless.

During its performance he got brandy at intervals dropped from a sponge on the tongue; his pulse got very slow, and his lips lost their colour two or three times, but brandy and ammonia brought him round each time. The operation was concluded at 11·15. Warm blankets were then wrapped round the patient, hot jars applied to his feet, and he got some warm brandy and water. Fifteen minutes afterwards he got twenty-five drops of sol. mur. morphia, in some brandy. The pulse was then 125; the colour of the lips was good; the temperature of the right leg and foot was lower than that on the opposite side. There was no paralysis whatever; on the contrary, he kept continually drawing up the left leg. He was very restless, wanting to loosen the bandages.

12 o'clock noon.—He got another draught of morphia and brandy. He is still restless, trying to throw off the bedclothes. He has quite recovered from the immediate shock of the operation. About every quarter of an hour he shouts out complaining of the burning pain in the ball of the right great toe, and in the sole of the same foot. This pain is only of a transitory nature, and subsides when firm pressure is made on the toe. Occasionally he felt, he said, the same pain a little above the right knee. These pains came on in paroxysms.

12·30.—Pulse 124. He is still restless, and complains of the pain and burning heat in the great toe. There is no external cause that could give rise to these symptoms.

1·30 p.m.—He got another draught of morphia and brandy. It is difficult to keep the bedclothes on him, he tries so frequently to throw them off.

2·30 p.m.—During last hour has taken some warm beef-tea with relish, and seems inclined to sleep. The temperature in the left lower extremity is very good; that in the right has greatly improved. The toes do not feel at all so cold as they did.

3·30.—Has had some sleep.

4·30.—No particular change. Seems still inclined to sleep.

5·30 p.m.—Just previous to this hour he had a semi-syncopal attack, from which he was roused by the exhibition of small quantities of stimulants. The pulse at the wrist became, during these attacks, almost imperceptible, the heart's action very weak, and the lips white. All these symptoms passed off. Then short semi-syncopal attacks recurred three or four times subsequently, from which, however, he always rallied on being given brandy and ammonia, &c.

9 p.m.—Pulsation in *left* femoral artery had returned; in the tumour no pulsation or bruit whatever.

At 10 p.m. it became obvious that his end was approaching.

10·30.—He is perfectly unconscious.

At midnight he expired.

On making the *post mortem* examination the following day, it was found that the aneurismal tumour engaged the lower part of the right common iliac, the external iliac, and also the upper portion of the femoral artery on that side. The tumour was firmly adherent to the surrounding structures, and had partially eroded the pelvic bones. The tumour contained a considerable amount of coagulated blood, and also some recently-deposited fibrine. About

an inch of the upper portion of the right common iliac was found free of disease, but owing to the overlapping of the tumour the vessel was to a great extent concealed from view, and what was seen was flattened out by the pressure of the tumour on it, which gave it the appearance of being very much dilated. For this reason, as well as from there having been extensive adhesions between the anterior wall of the artery, and the posterior wall of the aneurismal sac, the application of the clamp on any portion of the iliac artery was not deemed practicable. In truth, the adhesions were so intimate that the separation of the tumour from the artery, which it to so great an extent overlapped, was after death accomplished with considerable difficulty.

The wire compressor was found immediately above the bifurcation of the aorta. On removing the instrument, which was done with the greatest facility, and slitting up the vessel, which I did in the presence of Mr. Porter, Mr. Robert Mayne, and Dr. Hewitt, the artery was found without the slightest injury being done to any of its coats. No abrasion or any sign of injury was found in the vessel; a slight degree of paleness and a small amount of recently-formed fibrine being the only traces of any attempt at deligation. Before removing the compressor, and slitting up the vessel, I passed a stream of water down the aorta, none of which passed the instrument. The muscular structure of the heart was found pale and friable, and presented, under the microscope, evident traces of fatty degeneration. The microscopical examination was made by my friend and colleague in the Carmichael School, Dr. Purser. There was no evidence of any organic disease discovered in any other vital organ. No filament of a nerve was found to be pressed on by the wire, which, during the patient's life, I thought might have occurred, and been the cause of the severe pain which, after the operation, he so frequently complained of, and referred to the right foot.

The points of special interest and importance which we learn from this case are—

1st. That a vessel of such calibre as the abdominal aorta can be efficiently occluded without doing any damage whatever to the coats of the vessel.

2nd. That after the closure of this vessel the collateral circulation can be re-established in a very few hours.

3rd. That rapid consolidation of the contents of an enormous

false diffused ilio-femoral aneurism can take place after occluding the abdominal aorta.

4th. That death in this case was caused by the shock of the operation in a person with fatty degeneration of the heart.

5th. That if the patient had not had this condition of the heart, and been able to bear the shock of the operation and survive, the chances of gangrene subsequently occurring would have been small, from the fact of the rapid re-establishment of the collateral circulation, and the power we had of removing the occluding instrument at any moment and allowing the circulation to go on as before.

The particulars of the five cases preceding mine, in which the abdominal aorta has been deligated, are so well known to every well-informed surgeon, that I shall refrain at present from entering into the particulars of them. In the accompanying table will be seen the names of the surgeons who have performed the operation, the places where it has been done, the age of the patient, the date, and the form of aneurism for which the operation was performed. The cases which give most promise of the operation ultimately succeeding are those of Dr. Monteiro, Mr. South, and Sir Astley Cooper. In the first, the patient lived ten days, and then died from hemorrhage at the seat of the ligature. In the second the patient survived the operation forty-three hours; and in Sir Astley Cooper's twenty-three hours. The other patients died at shorter intervals.

Table of Cases of Deligation of Abdominal Aorta.

Surgeon	Hospitals	Sex and age of patient	Date of operation	Form of Aneurism
1. Sir A. Cooper	Guy's Hospital	Male, æt. 38	June 25, 1817	Diffused ilio-femoral aneurism
2. Mr. James	Devon and Exeter Hospital	Male, æt. 44	July 5, 1829	Aneurism of external iliac artery
3. Mr. Murray	Civil Hospital, Cape Town	Male	Oct. 4, 1834	Aneurism of external femoral artery
4. Dr. Candido Monteiro	Rio Janeiro	Male	July 5, 1842	False diffused aneurism of femoral artery
5. Mr. South	St. Thomas' Hospital	Male, æt. 28	June 21, 1856	False diffused aneurism of common iliac artery

Looking at the formidable nature of an operative procedure which necessitates the exposure and occlusion of a vessel of such magnitude as the abdominal aorta, and the want of success which

has hitherto attended such procedures, justly described by the late Mr. James of Exeter, as the "most formidable that could be conceived," it is not surprising that so few operations of the kind have been undertaken by practical surgeons. This want of success, however, should not lead us to abandon the hope that the operation may yet be the means of saving, as it has been of prolonging, life, and adding another to the long list of brilliant achievements that have been already accomplished by surgical art. May we not hope that ultimately the operation will succeed, when the illustrious Astley Cooper, who, as a surgeon, was as philosophical as he was bold, in speaking of the case, in which he, for the first time in the history of surgery, applied a ligature to the abdominal aorta, observed, "if a case were to offer itself similar to that I have told you of, I would immediately perform the operation again; and *my own conviction is, that it can be done, and done with success.*"

ART. II.—*On the Differential Diagnosis of Abdominal Aneurism.*[a] By WILLIAM MOORE, M.D., Univ. Dub.; "King's Professor of the Practice of Medicine;" Physician to Sir P. Dun's Hospital, &c.

OF late years great advances have been made in the diagnosis of thoracic aneurism and intra-thoracic tumours, through the combined application of physiology and pathology; and the same branches of science, when carefully applied to the diagnosis of abdominal aneurisms and tumours, have thrown increased light on their detection. I am happy to see among us in health and vigour a gentleman who was one, if not the first, to give us an accurate detailed account of the *symptoms* of abdominal aneurism. Some forty years ago Dr. Beatty pointed out the deep-seated pain in the back, with neuralgic exacerbation; the agonizing intestinal pain, with occasional complete intermissions and other symptoms, to which I shall allude more fully hereafter. These phenomena were present in a case under Dr. Beatty's care, which proved fatal on the 26th of February, 1829, and in which a large aneurism was found, covered by the crura of the diaphragm, which were expanded and tightly stretched over the sac. The aneurism lay on the three last dorsal vertebræ, the bodies of which it had deeply eroded.

[a] Read before the Medical Society of the King and Queen's College of Physicians.

I shall now bring before you the details of two cases of abdominal aneurisms which exemplify symptoms and physical signs, some of which, as far as I am aware, have not been noticed heretofore in connexion with abdominal aneurism.

The first case[a] was that of James L., aged thirty-two, who was admitted into Sir P. Dun's Hospital on the 31st December, 1868, complaining of pain in the back. For many years this man had enjoyed excellent health, although occasionally he had led an intemperate life. For some years before his death he had earned his livelihood by assisting to coal the "Cunard" steamships; and as he was paid according to the amount of work done, he laboured very hard, often for a day and night continuously. About three years ago, whilst engaged in this laborious occupation, he fell into the hold of one of the ships, and shortly after, for the first time, he complained of pain in the back. He then came to Dublin and placed himself under Mr. Tufnell's care for about three months, when the epigastric pulsation, which he had accidentally observed a short time before, disappeared, and the pain across the back and loins abated. He resumed work, but was soon obliged to give it up, and again to seek Mr. Tufnell's advice. Feeling relieved he again returned to Liverpool, where he was afterwards treated by Dr. Waters.

On admission into Sir P. Dun's Hospital he looked pale and spanemic, still his general health was pretty good. He was of rather an excitable temperament. His sight, speech, and hearing were unimpaired, but his voice was low and subdued. The respiration was regular; he was neither jaundiced nor dropsical; his temperature was normal. The urine showed a healthy reaction and specific gravity, and contained no albumen. He made one complaint, viz., of a severe pain about the last dorsal vertebræ, which at times radiated under the floating ribs on the left side.

On examination there was a pulsation just visible in the epigastrium; in character eccentric, equable, and persistent in all positions; there was dulness on percussion, and a systolic bruit could be heard over the seat of the pulsation when the patient was in the recumbent, but which became inaudible when he assumed the upright position. The pulsation, in the iliac arteries was feeble, but equal. There was increased dulness over the region of the heart, and a peculiar diastolic "click" supplanted

[a] Reported by Mr. Richard W. White, Clinical Clerk.

the second sound. There was visible pulsation in the radial arteries, and the pulse was 80.

On placing the patient on his face no appreciable dulness could be discovered along the spine, nor was any pulsation to be seen. On examining him with the stethoscope from the nape of the neck downwards, no abnormal sounds could be heard till we came down to the *last dorsal vertebra*, *when a well-marked bruit became audible* (particularly to the left of the spine), which became louder as we descended for about two inches, when it "shaded off," and was lost about the fourth lumbar vertebra. I should say the area of the "bruit" was about five inches.

The patient had a slight bronchitic attack on admission, which was soon relieved. On the day of his death he was unusually cheery. When he suddenly called "to send for the doctor," but before Mr. Gregg, the resident medical scholar, could be brought to him, he was dead.

The *post mortem* examination was made most carefully by Mr. Gregg, assisted by Mr. Wall. In the lungs no lesion was found further than a congestive condition, and some frothy mucous in the larger bronchi, such as we would expect to find from the suddenness of death. The heart was hypertrophied, but the valves were perfect. Atheromatous deposit were present here and there over the thoracic aorta, but there was no dilatation of the vessel. On following the vessel downwards immediately opposite the giving off of the celiac axis, an aneurism was found about the size of a small apple fitting into the left vertebral hollow and lying on the upper lumbar vertebræ, the bodies of which were eroded. The aorta was narrowed for about half an inch below the giving off of the aneurism; the right renal artery was obliterated, and the right kidney was reduced to about one-third its normal size.

In this case the two most prominent symptoms were pulsation in the epigastrium and complaint of pain in the back.

Now, pain in the back and lumbar region may attend abdominal tumours of any kind from mere pressure; hence the differential diagnostic value of this symptom must be taken for what it is worth. Again, with how many affections and under how many circumstances do we meet with abdominal pulsations. Tumours of various kinds overlying the great vessel will carry pulsations, so may an hepatic abscess, or even fecal accumulations in the transverse colon, whilst, in addition, we meet with hysterical

pulsation in spanemic men and women, and pulsations connected with aortic regurgitation.

But we are told by some authorities that the pulsation of aneurism can be diagnosed by its eccentric and expansile character. Now, I think too much stress is laid on this "expansile" character of the pulsation. In the case I have just detailed, no doubt the pulsation was slow, strong, and decidedly expansile; still one of the most eccentric and expansile pulsations I ever met with attended a case of hepatic abscess. For my own part, I consider "persistency and equability," after a careful examination in all positions, the most characteristic features of abdominal pulsation; consequently, when we meet with an abdominal tumour of any kind carrying pulsation in the supine position, place the patient on his or her hands and knees, when the pulsation, if due to an aneurism, will persist, but if due to a tumour overlying the vessel, the pulsation will disappear from the tumour falling off the vessel, unless they are firmly attached by adhesions.

Let us next analyse the stethoscope signs in this case. When the patient lay on his back a systolic bruit could be heard over the seat of the pulsation, which was lost when he assumed the upright position. But here, again, we are met with the fact that a systolic murmur can usually be heard over the abdominal aorta in front, in cases of tumour of any kind overlying the vessel, when the patient is in the horizontal position, and which disappear or are lost when the patient assumes the erect position from the tumour falling off the vessel. Hence the differential diagnostic value of stethoscopic signs heard over the abdomen *in front*, must be taken for what they are worth. But if we place the patient in the *prone* position, and examine him along the spine, in the great majority of cases of aneurism a "bruit" will be heard (as in the case detailed) along the spine, especially to the left side. In the case of an abdominal tumour, with the patient in the *prone* position, no such "bruit" will be heard, inasmuch as no pressure is exerted on the vessel from the tumour falling forward. Thus, in a "*localized bruit*," *heard along the course of the spine*, we have one, if not the most valuable, physical sign of abdominal aneurism we are as yet possessed of.

But again, you may say, do we not meet with a "bruit" along the spine in a case of "obstructive disease" of the aortic orifice and in "spanemia," and therefore how will this physical sign aid us in the differential diagnosis? Well, in the case of aortic

obstruction, the "bruit" usually is not propagated so low down as the lumbar region, but if it is it will be audible *continuously* from the base of the heart along the aorta. *Continuity* would also be the characteristic of a spanemic murmur; whereas, in the case of aneurism the "bruit" is *localized*, the area of its presence, of course, being regulated by the "vis" of the circulation, the nature of the foramen of communication, the state of the sac, and other such pathological conditions. For my own part, I regard a *localized* murmur, heard along the spine in the horizontal position, the most pathognomonic sign of aneurism we are possessed of, and I should advise its being looked for in all cases of continued lumbago or chronic lumbar pains, as I have no doubt this "bruit," if carefully looked for, might have been the means of disclosing many cases of aneurism which were not suspected—cases in which no visible or objective signs of the disease were present over the front of the abdomen. In this view I am happy to have the concurrence of Dr. Beatty, who, in connexion with his interesting case, goes on to say, "he regrets that they were not led to employ auscultation to the spine, as I think it very probable the disease would thus have been discovered."

The second case of abdominal aneurism which I have comparatively lately had an opportunity of seeing, occurred in one of our most worthy members, and whose death was deeply lamented by us all.

On the evening of the 29th of October last, I was suddenly summoned to Blackrock to see the late Dr. H——. I found him writhing with pain; his groans could be heard in the adjoining rooms; he was almost pulseless; his extremities were cold, and he had a ghastly "look," which might be well described as "death in his face." He entreated of me to give him some relief from an excruciating pain in the abdomen and back, which he had been suffering from almost uninterruptedly for the previous forty-eight hours. I at once administered 50 drops of the solution of morphia; hot fomentations were applied to the abdomen and extremities, and he got brandy freely. In a short time he expressed himself relieved, and he fell asleep. I remained with him till about seven p.m., when I left him, as Dr. Pollock, under whose care he had been for the two days previous, was to visit him about eight p.m. Before leaving I advised that he should get a grain of opium and hot brandy punch if the pain returned with severity. When Dr. Pollock arrived he found Dr.

H. sleeping; he awoke soon after, and was again seized with the excruciating pain, and died almost immediately. I need hardly add that under such circumstances the making any minute physical or stethoscopic examination was simply impossible.

I was unavoidably prevented from being present at the *post mortem* examination, which was most carefully made by Dr. E. Hamilton, assisted by Dr. Pollock; however, Dr. Hamilton has kindly given me the details of the examination, which, in general terms, showed a large aneurism arising very high up between the pillars of the diaphragm. This aneurism had burst into the peritoneum, filling the lower part of the abdominal cavity with blood; it had eroded the bodies of the upper lumbar vertebræ to some extent. As the cause of death was so apparent no examination of the other cavities was made.

Now, it may be interesting to review the symptoms which Dr. H. suffered from for some years previous to his death; and as I was frequently consulted by him for many years past, I am enabled to give you a general outline of his case.

About eight years ago Dr. H. had an aborted fever with bronchitis, in which he was attended by Drs. Stokes, Croker, and myself. From this he soon recovered; but as the cough persisted, we recommended him to go to the south of Europe for some months, which he did. He returned to Dublin about the middle of May following—and here I may mention that in passing through London he consulted Dr. Williams, who told him there was no sign of pulmonary disease about him. He then resumed the active duties of his profession; at times the cough seemed to disappear; he gained flesh, and for the last seven years his weight must have been on an average between fifteen and sixteen stones. However, within the last few years a cough of another character seemed to me to ensue; it was dry, barking, and spasmodic, and from time to time he suffered from "aphonia," for which he consulted Dr. Smyly, who tells me that the laryngoscope showed an œdematous condition of the vocal cords. From these attacks of temporary aphonia he always recovered, but latterly they seemed to recur more frequently and lasted longer, and at the time of his death he had just regained his voice from an attack of complete aphonia. He never made any complaint of pain referable to abdominal aneurism,[a] if I except the complaint of lumbago, which

[a] I have since learned that Dr. H. did complain of pain in the back to others.

he casually made to me on one occasion *only*, about a month before his death. Indeed, I can say with perfect certainty that he had not the slightest suspicion that he was labouring under such a formidable malady.

The question suggests itself, what connexion, if any, existed between the thoracic symptoms, viz., "cough," "dyspnea," "aphonia," and abdominal aneurism, or would not the aphonia and dyspnea be more accurately accounted for by pulmonary phthisis? I think I will be able to show you that in Dr. H.'s case these thoracic symptoms were not due to phthisis, but symptomatic of abdominal aneurism. First, of the aphonia: it was of an ephemeral or temporary character, and recurred from time to time. Often the loss of voice was complete, it differed from the aphonia of laryngeal phthisis in these respects, the loss of voice in phthisis being usually a gradual process; but once established, in most instances, being permanent. In Dr. H.'s case the *variations* of the voice within certain periods, and the temporary character of the aphonia, seems to be explicable by pressure on or irritation of the recurrent laryngeal nerves. Again, the cough was of a semi-laryngeal, dry, barking, spasmodic character, accompanied with dyspnea; in these features it differed from the cough of chronic phthisis; in addition, there was no general wasting, for at the time of Dr. H.'s death he must have weighed between fifteen and sixteen stones; the only part of his body which did look pinched was his face, and in chronic phthisis the face is usually the last part of the body to show dyscrasia. Further, from the most careful inquiries, I do not find the family history to be phthisical; his father died comparatively advanced in life of some abdominal disease, probably aneurism, whilst his mother lived to a very advanced age.

Another point suggests itself, might there not have been an aneurism of the thoracic aorta giving rise to the symptoms above mentioned? Well, that is possible, as no *post mortem* examination of the thorax was made. But if an aneurism of the ascending or transverse portion of the aorta had existed, in all probability there would have been complaint of neuralgic pains over the chest; or of "angina," or of "dysphagia;" or some difference in the pupils, or in the pulse at the wrists, or increased venous radiation over the chest, or dulness on percussion, with a second centre of pulsation with "bruit;" or if aneurism of the descending portion of the arch had existed, interscapular pain would probably have been

complained of; there would also very likely have been persistent dyspnea from pressure on the bronchus, or deficiency and inequality of respiration in either lung, with eventual diminished capacity of the side. Now, not one of these symptoms or physical signs were present.

Well, if these thoracic symptoms were not due to disease of the lungs or great vessels in the chest, how can they be explained in connexion with the abdominal aneurism in this case? If we reflect for a moment on the seat of the aneurism, we find it surrounded by the greatest prevertebral plexus of nerves in the whole body, the semi-lunar ganglia of the solar plexus, in which plexus we have the connexion with the sympathetic system in the chest kept up through the medium of the greater and lesser splanchnic nerves; and as far as we know at present, the pneumogastric nerve terminates in the same great central plexus. Now, pressure exerted on this great ganglionic centre is reflected on the larynx through the pneumogastric and its tributary the recurrent laryngeal nerve; hence the "aphonia," whilst the cough and dyspnea are explicable in the same reflex manner, inasmuch as the pulmonary plexus is mainly made up of the pneumogastric and sympathetic, and no doubt the phrenic nerve contributed to the dyspnea from the high position of the aneurism and consequent pressure on the diaphragm. In this case, therefore, the prominent symptoms of abdominal aneurism were *thoracic*. The only symptom pointing to disease of the abdominal aorta was the *transient* complaint of lumbago made to me on one occasion *only*, as I have already mentioned, about a month before his death, when he was suffering from complete aphonia. You may naturally inquire if I can adduce any analogous cases to endorse the explanation of the thoracic symptom I have thus imperfectly detailed to you. I answer I can, and for this purpose I will again allude to Dr. Beatty's case, to which, as I have already said, we owe so much. Now, in the comments which have been made in Dr. B.'s case (and they have been very numerous), *all* importance is attached to the pain in the back, neuralgic exacerbations, &c.; little or no value seems to have been given to *two* symptoms, viz., "dysphagia" and "dyspnea," after the patient took fluid, symptoms which, considering the great advance that has been made in the diagnosis of latent tumours through the medium of reflex nervous symptoms, seem to me of the greatest interest and importance. In Dr. Beatty's case the situation of the

aneurism would not have given rise to mechanical pressure on the œsophagus, nor could it have pressed on either bronchus, for if so these symptoms would have been persistent; and, in addition, continued pressure on either bronchus would have entailed a difference in the respiration in the respective lung which, from the careful examination of the chest which was made by so many distinguished stethoscopists, no doubt would have been discovered. Hence as direct pressure will not explain the symptoms of dysphagia and dyspnea in this case, to what then could they have been owing? Manifestly to reflex nervous action of the pneumogastric. And if so, Dr. B.'s case would go to show that "dysphagia" may be added to "aphonia" and "dyspnea," as a symptom of abdominal aneurism.

But I can adduce another case of abdominal aneurism in which a symptom of very great interest in a comparatively remote organ occurred, and which the author considers was due to the presence of the aneurism. I refer to a case of abdominal aneurism, read by Dr. Seaton Reid before the Belfast Pathological Society, and published in the *Dublin Hospital Gazette* in 1860. It was the case of a man, aged forty, who had been employed lifting heavy weights. The *post mortem* examination showed that, with the exception of some atheromatous deposits, the calibre and coats of the aorta appeared normal until within an inch and a half of the origin of the ciliac axis, where it became dilated to nearly double its natural size, and continued so for about an inch below it. From the vertebral surface of this dilatation, two sacs, or perhaps a bilocular aneurism, were given off; that on the left side of the vertebra was about the size of a cricket ball, whilst that on the right was the size of a large cocoa nut, and had dissected its way between the diaphragm and the pleura, extending into the cavity of the chest fully six inches; and perpendicularly to the same extent, firmly attached to the bodies of the four last dorsal and two upper lumbar vertebræ. Over the anterior portion of this large sac the great splanchnic nerve could be seen, stretched; there was a slight amount of dilatation of the innominata. The *right pupil* was markedly smaller than the left.

Dr. Reid remarks from the slight amount of dilatation of the innominata and the healthy state of the aorta in this patient, there was no possibility of pressure being made in the cervical region, and therefore we are forced to inquire if the contracted state of the pupil might not have been owing to the pressure that must have

been made on that portion of the sympathetic that forms the great splanchnic nerve, by its being stretched to such an extent over this large aneurism on the right of the vertebra. I shall not occupy your time further on the present occasion than by drawing what seems to me to be practical inferences from the two cases I have brought before you. The first exemplifies a physical sign of aneurism which heretofore has not been sufficiently appreciated and looked for, viz., the presence *of a "localized" bruit along the course of the spine.* It seems to me to be the most pathognomonic sign of abdominal aneurism we are as yet possessed of, and in all cases of long-continued pain in the back and lumbar region it should be carefully looked for.

The second seems to me instructive, as showing that cases of abdominal aneurism may occur in which the symptoms, which heretofore have been considered especially characteristic of the disease, may be absent—the most prominent symptoms being *thoracic.*

ART. III.—*Thoracic Aneurism.* By THOMAS HAYDEN, M.R.I.A., Fellow College of Physicians; Physician Mater Misericordiæ Hospital; Professor of Anatomy and Physiology Catholic University, &c.

WILLIAM FAY, aged twenty-eight, a cabman, of intemperate habits, first came under my notice about January, 1866, suffering from chronic laryngeal symptoms, manifestly the result of his irregular life. After a short course of treatment in hospital he was restored to his ordinary health, and discharged. Shortly afterwards he strained his back in lifting his cab, and then, for the first time, complained of pain in the lower dorsal spine. He became an extern-patient of the Mater Misericordiæ Hospital on the 22nd of June 1868, and was re-admitted shortly afterwards. He then suffered from severe pain of a paroxysmal character in the epigastrium, and extending to the sides and back; pulse 84 and weak, but regular, and equal on the two sides; pupils normal and equal; severe pain in the left gluteal region; cardiac impulse strong; in the scrobiculus cordis a loud bellows murmur was heard, which was synchronous with the pulsation of the abdominal aorta. Occasionally a second murmur was likewise heard in this situation, diastolic in time, cooing in character, and resembling very closely a musical venous murmur.

1, 2. Superior lobe of left lung overlapping and attached to the heart. (1. Lung-tissue in section).
3. Heart (pericardium removed) occupying pulmonary fissure, and flattened by pressure.
4. Inferior lobe of lung flattened out and incorporated with aneurismal sac (seen in section).
5. Aneurismal sac, torn in process of removal.
6. Jagged and coloured fibrin occupying aneurism.
7. External surface of sac formed by thickened pleura.
8. Empty portion of sac.
9. Diaphragm expanded upon and attached to sac.
10. Opening in aorta (two probes introduced into the vessel).
11. Everted portions of torn sac.
12. Left kidney.

The former was faintly heard over the border of the left costal cartilages near the xyphoid, but evidently by transmission. The treatment consisted in iron in a bitter infusion, aperients, and a small blister to the gluteal region.

Discharged, much relieved July 31st, and again admitted August 14th. For two days after leaving hospital he had been entirely free from pain, but during the remainder of the period, to the date of re-admittance, had suffered from excruciating pain in the left side of the abdomen, shooting down to the left hip, and also to the left groin and knee in the course of the ilio-scrotal and anterior crural nerves; it was paroxysmal, causing a "burning" sensation in the left groin and back, and was aggravated when he assumed the erect posture. A *bruit de soufflet* was heard beneath the ensiform cartilage, synchronous with the pulsation of the abdominal aorta; it was soft and "lisping" in character, traceable upwards to left of ensiform cartilage, where both sounds of the heart were likewise audible; here it was heard to follow the first sound, but at an appreciable interval of time, whilst it immediately preceded the second sound without any interval. After a voluntary movement of the patient's body the murmur was likewise audible at the apex of the heart, where it was fainter, but otherwise unaltered, and held the same relationship to the sounds. Its point of maximum intensity was at the tip of the ensiform cartilage; here it was constant, and when audible in all three situations, might be followed from below upwards and to the left in an uninterrupted line of about three inches, but with progressively decreasing intensity; it was not audible elsewhere in front; was independent of respiration; above the level of the xyphoid cartilage it was occasionally suppressed during one or two pulsations; and on two several occasions the most careful examination in the recumbent posture failed to detect it beyond the limits of the scrobiculus cordis. Occasionally a second murmur was heard immediately succeeding the former, and therefore coincident with the systole of the aorta; it was musical in character, and strictly localized. Pressure upon the abdominal aorta at any point below the seat of murmur had the effect of diminishing the force of the principal, and of suppressing the second or after-murmur. The heart pulsated in its usual situation, and the area of precordial dulness, and the cardiac sounds, were normal.

To have two leeches to seat of pain in epigastrium.

August 23.—No murmur audible anywhere in front, save in the

scrobiculus cordis, where it is double; the latter element occasionally musical.

August 24.—The principal murmur is again audible over line from apex of heart to scrobiculus. Over the last dorsal and first lumbar spinous processes, and an area of about two inches in radius, having this point as centre, there is heard a loud blowing murmur, but no impulse is perceptible; this part is tender on pressure, and the seat of "shooting," "burning" pain. To have two leeches applied here.

On the 2nd September the pain in the left side of the abdomen and back was excruciating; the patient implored us to relieve him, and one-twelfth of a grain of muriate of morphia in solution was injected subcutaneously in the former situation, with the effect of completely subduing pain for a period of twelve hours.

The following remarks, written under date of September 3rd, are copied from my note-book.

The collateral evidence here is sufficiently strong to warrant the diagnosis of inferior thoracic, or thoracico-abdominal aneurism, even in the absence of tumour and perceptible impulse. No doubt exists in my mind as to the non-cardial nature of the murmur heard over the left costal cartilages, and as to its identity with that heard beneath the ensiform cartilage, and synchronous with the pulsation of the abdominal aorta, though modified by distance from the seat of its origin.

The interval of time that must elapse between the systole of the left ventricle, and the manifestion of its effects by impulse and murmur in an aneurism of the inferior thoracic, or superior abdominal aorta, though infinitesimal, is nevertheless strictly calculable from a knowledge of the distance between the points mentioned, and of the rate at which the blood-wave moves in the individual under examination; if now the interval so determined be affixed to the period of the first sound of the heart, and the abdominal murmur which "times" it, supposed to be transferred or transmitted to the area within which the cardiac sounds are audible, it is manifest that the murmur will be heard in the precordium at the close of that interval, and that it must coincide with some one cardiac phenomenon posterior in time to the first sound.

Now, if that phenomenon happen to be the negative one of the "short pause," and the terminal portion of it, we shall have a murmur not belonging to the heart, audible in the precordium, and coinciding with that portion of the short pause immediately anterior

to the second sound. Such a murmur I would designate as *pre-diastolic*.

In the case under consideration, if one hand of the observer be placed upon the abdominal aorta, and the other in the site of the apex-pulsation, whilst the ear is applied to the stethoscope placed below the ensiform cartilage, the asynchronism between the impulse of the heart upon the one hand, and the pulsation of the aorta and the murmur on the other, will be readily appreciated.

Murmurs of this precise rhythm are, as far as I have observed, invariably exo-cardial.

The patient's condition underwent some improvement, and he was discharged in the early part of October. I now lost sight of him till March, 1869, when I visited him at his lodgings, off Lower Dorset-street. He was then coughing up unmixed florid blood in considerable quantity; pulse 120, regular and equal; pain was now referred chiefly to lower part of left side of chest, which was absolutely dull, and devoid of vocal thrill and respiratory sound; the intercostal spaces were here obliterated, but tender to pressure, and the hand placed on this portion of the chest perceived a general heaving movement.

From the angle of the scapula upwards respiration was normal, but accompanied with râles; a faint rough systolic[a] murmur was audible at the ensiform cartilage; the heart pulsated above the level of the nipple; its action was strong; and its sounds loud, clear, and normal.

A blister was directed for the side, and acetate of lead in gr. v. doses as a styptic. On the 18th March, he was again admitted into hospital, and on the 19th the following note of his condition was taken. Pulse 120; left side absolutely dull at all points below level of second intercostal space, and devoid of respiratory and vocal phenomena; impulse here not distinctly to be felt; above level of third rib percussion-resonance is of a hollow tympanitic character; in front this part of the chest yields no respiratory sound, but posteriorly it is the seat of bronchial respiration with fine crepitus; over posterior inferior portion of left side two sounds are heard, but no murmur; and at the scrobiculus cordis a murmur is audible only on making firm pressure with the stethoscope. The patient was sleepless, and expectorating viscid pneumonic sputa; the lips were

[a] *i.e.*, representing the *systole* of the left ventricle in a *distant* artery, in the sense previously explained.

dotted with the crusts of dried herpetic eruption. To have 1½ gr. of aqueous extract of opium at night.

On the 9th April it was noted that the left infraclavicular region, from the median to the axillary line, had for some days previously yielded a hollow tympanitic sound, with variable respiration, which at one time was loud and puerile, with coarse muco-crepitus, and on the following day all but suppressed; these alternations were of frequent occurrence; the heart was now displaced upwards, and pulsated visibly and tumultuously in the second and third left intercostal spaces, near the margin of the sternum; these spaces, nevertheless, yielded a hollow sound on percussion; the entire left side was enlarged, and the inferior costal cartilages of that side projected considerably; there was likewise a prominence in the inferior axillary region, and the patient was troubled with a loud ringing cough. The upper piece of the sternum, with the corresponding extremities of both clavicles, projected sharply, but this projection had existed from childhood, and was due to the natural conformation of his chest; there was no unusual dulness in this situation, but at the acme of expiration an obscure heaving pulsation was perceived by the hand placed flat upon it; and over its lower and right portion, corresponding to the attachment of the second costal cartilage to the sternum, and strictly limited to a space of about two inches in diameter, a harsh murmur, prediastolic in rhythm, was heard. The cardiac sounds were loud and free from murmur. The epigastric hollow was obliterated; the liver projected considerably, but equally; its surface was even; the organ was not displaced downwards, nor did the hepatic dulness extend above the level of the sixth rib. Expectoration copious and purulent, and occasionally mixed with a little blood.

April 29.—The secretion of urine has been greatly diminished since last report, amounting for the last fortnight to not more than eight ounces in the 24 hours; it is loaded with lithates; pulse 120, weak but regular; respiration 48; cough troublesome and ineffective; loud puerile respiration for two inches below the left clavicle; a faint murmur synchronous with abdominal pulsation existed at the ensiform cartilage, where the sounds of the heart are faintly audible, but from this point upwards to the site of cardiac pulsation they increase progressively in distinctness. On the preceding night the patient was attacked with aggravated dyspnea, accompanied with severe pain in the epigastrium, and shooting down to the left groin. To have a sedative of hydrocyanic acid and morphia at night.

On the 6th of May a loud bellows murmur, nearly coincident with the radial pulse, was detected in the sitting posture along the lumbar spine, and for two inches to the left; in this situation there was severe and fixed pain which was not aggravated by pressure or percussion; prediastolic murmur remarkably loud and harsh over sternal prominence and to right of its base, in both of which situations the hand can detect a heaving impulse; between the site of this murmur and that of cardiac impulse no murmur was discoverable; neither did murmur exist in connexion with the cardiac sounds. The pulsation of the heart was violent, lifting up at each stroke the corresponding portion of the chest-wall, which yielded a hollow sound on percussion. No respiratory sound was audible in left infraclavicular space; there was some fetor of the breath. To have compound decoction of scoparium with nitrous spirit of ether; also an anodyne at night.

May 9.—During my visit to the ward at 12 o'clock he was suddenly attacked with the most distressing dyspnea, which caused him to start into the sitting posture; respiration was very rapid and laboured, and the cervical veins became turgid, but the face remained pale. On inquiry I learned that in the course of the preceding fortnight he had had several such fits, but none of so aggravated a character as this; there was great anxiety, and the patient besought me not to leave him. The dyspnea continued, unmitigated by ethers and stimulants; the patient became gradually weaker, and died of exhaustion at two o'clock. On the evening of that day a *post mortem* examination of the body was made by the resident pupils, Messrs. Furlong and Kelly, under circumstances of much difficulty. To these gentlemen, and to Mr. Petit, one of the present residents, I am indebted for the opportunity of exhibiting the morbid specimen to the society,[a] and of clearing up some doubts that arose in my mind in the course of the case; as well as of elucidating many phenomena of great interest in regard to the history and diagnosis of aneurism of the thoracic aorta.

The haste with which the examination was made will account for some injuries sustained by the viscera during removal.

On removing the sternum a good deal of serum escaped; whence this proceeded it is difficult now to say, but it probably came from the right pleura. The great veins at the root of the neck, and the cavæ, were distended with blood; the heart lay across the chest

[a] Medical Society, Wednesday, May 26, 1869.

below the left clavicle, its apex to the left and its base to the right side; it occupied the great pulmonary fissure of the left lung, displacing upwards, and pressing into the cone of the pleura, its superior lobe, the base of which overlapped, and in great measure concealed it from view; it was rather below the average size of the adult heart, and was perfectly sound in its walls and in its valves; the arch of the aorta was free from disease, but the curve at the junction of its ascending and transverse portions was greatly exaggerated owing to the displacement of the heart upwards; the superior lobe of the left lung was universally and firmly attached to the parietal pleura, and much reduced in volume by compression between the displaced heart and the superior osseous boundaries of the chest, but it was otherwise free from disease; the inferior lobe of the lung was entirely obliterated as such; it was converted into a thin lamina spread out upon, and firmly attached to, the surface of a vast solid tumour which occupied nearly the entire of the left side of the chest. This tumour was found to be an aneurism communicating with the thoracic aorta immediately above the diaphragm, by an oblong opening about two inches by one inch in diameter, and engaging one-half the circumference of the vessel; the edges of this opening were rounded off and expanded into a vast aneurismal pouch which extended vertically from the second rib to the left kidney, displacing the lung and heart upwards, the diaphragm downwards, the liver forwards, and the ribs and costal cartilages outwards. It was firmly attached to the chest-wall from which it was removed with great difficulty, and inseparably so to the diaphragm, beneath the external arched ligament of which it had descended behind the left kidney, and through which it was attached to the upper extremity of that organ. The sac of the aneurism, which was formed by condensation of the surrounding tissues, was lined on three aspects by dense laminated fibrin, viz., inferiorly, externally, and posteriorly; but superiorly, and also internally, it was composed of little more than the pleura; whilst anteriorly, as already stated, it was strengthened by the condensed and adherent lung-tissue. The sac contained a great quantity of dark clotted blood, and in its upper part a mass of jagged fibrin mixed with coagulum. There was no opening from the sac save that which led into the aorta.

The bodies of the sixth, seventh, and eighth dorsal vertebræ were deeply and extensively eroded; the seventh had been almost entirely removed. The corresponding intervertebral fibro-cartilages were scarcely affected. The æsophagus trachea and bronchi had escaped

pressure; the right lung was healthy, as were likewise the liver, spleen, and kidneys; but the left renal artery was pressed on and nearly obliterated by the tumour.

In cases of this kind little can be done curatively; indeed nothing beyond mitigating the sufferings of the unhappy patient. The interest connected with such cases, therefore, has reference mainly to diagnosis.

The diagnostics of thoracic aneurism are still notably defective, although, in consequence mainly of our more precise knowledge of the special significance of symptoms, few physicians would now endorse the statement of Laennec, that "in the present state of our knowledge there assuredly exist no certain means of ascertaining the existence of this disease (thoracic aneurism) until it shows itself externally."[a]

A short *résumé* of the case will form the most suitable introduction to the few remarks I propose to make upon it.

A young man of intemperate habits strained himself in lifting a weight; shortly afterwards he complained of pain in the back, and two years subsequently he presented himself suffering from acute pain in the lower part of the back, and extending to the left side of the chest, the left hip and groin. This pain was intermittent and paroxysmal; and described as being of a "burning" "boring" character, and aggravated by the patient's assuming the erect posture. A murmur was heard at the ensiform cartilage, posterior in time to the first sound of the heart, anterior to the second sound, and synchronous with the pulsation of the abdominal aorta; a second murmur of a musical character, and coinciding with the second sound of the heart, was also occasionally heard in this situation; the former of these murmurs was modified, and the latter suppressed by pressure upon the abdominal aorta below the seat of their origin; a single bellows murmur was audible over and in the vicinity of the last dorsal and first lumbar vertebræ, which were the seat of pain and tenderness, and the centre whence pain radiated into the chest and abdomen; no impulse or murmur was discoverable in the epigastrum. The action and sounds of the heart were normal; there was no febrile excitement; vital functions performed satisfactorily, and general nutrition unimpaired. Five months subsequently the left side of the chest was found enlarged, and dull on percussion from the base to the angle of the scapula; to the same extent respiration and vocal vibration

[a] On the Chest, Forbes Translation, page 678.

were abolished; the heart pulsated above the left nipple; respiratory sound was heard superiorly, and here percussion sound was hollow and tympanitic.

No direct communication was discovered between the aneurism and the bronchial passages; and I incline to think no such at any time existed. The hemoptysis of March would, in that case, remain to be accounted for by transudation, consequent upon the pressure to which, at that time, the left lung had begun to be subjected by the aneurism.

The variable character of the respiratory and percussion sounds over the apex of the left lung, and coeval with dulness and suppressed respiration inferiorly, constitutes a feature of much interest in the case.

Some years ago I published[a] the details of a case in which a similar alternation of phenomena was exhibited, and on *post mortem* examination of the body the pleural cavity was found full of serum, the greater portion of the lung compressed and carnified, and the apex, which was healthy and resonant, firmly attached to the anterior wall of the chest and cone of the pleura.

In the progress of this case it was noticed, that the return of respiratory sound in the apex of the lung always coincided with increased flow of urine, and partial subsidence of the liquid accumulation in the pleura; or with paracentises abdominis and descent of the diaphragm. The explanation offered at the time was, that the varying degree of liquid pressure upon the external surface of the lung where it was unattached, *i.e.*, posteriorly and inferiorly, gave rise to a corresponding variation in the volume, and in the circulation of air within it, and hence the alternation of phenomena mentioned. Thus, when the pressure was considerable the air became static or motionless in that portion of the lung from which it had not been entirely expressed, viz., the apex; and where the latter was adherent to the chest-wall a hollow sound, or that of stationary air, was elicited on percussion, and vesicular murmur was suppressed, and bronchial respiration substituted. When, on the contrary, air was allowed to circulate in the apex of the lung by partial subsidence of the pleural effusion, whether due to increased activity of renal secretion, or removal by tapping of the fluid from the peritoneum, normal resonance and respiratory murmur were restored in that portion of the organ.

[a] "A case of ascites with anomalous thoracic signs."—Medical Press and Circular, April 4, 1866; Reports of Pathological Society, April 21, 1866.

In the case which forms the subject of this communication I venture to think that, *mutatis mutandis*, a similar explanation of the phenomenon may be offered; the entire lung was condensed, with the exception of the apex, which was attached by old adhesions to the wall of the chest, and subjected to pressure varying in degree with the volume of the aneurism, the latter being occupied chiefly by fluid blood. The partial suppression of urine, which still further complicated the case towards its conclusion, was probably, in some degree at least, the result of the direct pressure of the aneurism upon the left renal artery. The observations and experiments of Dr. Robinson[a] are of interest in connexion with this feature of the case.

The patient was pale; expectorating pure florid blood, but free from febrile excitement; his pulse was quick and feeble.

Three weeks later the liver was found pushed forwards into the epigastrium, but not increased in volume; the heart was displaced upwards beneath the clavicle; the ribs were everted, and the inferior intercostal spaces obliterated; a heaving pulsation, accompanied with a double sound, was perceptible over the inferior posterior portion of the corresponding side. The murmur at the ensiform cartilage persisted, but was then much less distinct; a bellows murmur, slightly posterior in time to the radial pulse, was heard along the lumbar spine; renal secretion was partially suppressed; the patient became gradually weaker; paroxysmal dyspnea supervened, and in one of these fits he expired.

The foregoing sketch, viewed in its entirety, no doubt leads necessarily to the diagnosis of aneurism of the inferior thoracic aorta; but it is no less manifest that certain portions of it, not interpreted by the light of the remainder, might seem to warrant a different conclusion. Thus, for example, if the patient were seen by a medical man for the first time when the left side of the chest was enlarged, dull on percussion, and devoid of respiratory and vocal phenomena throughout the greatest part of its extent, whilst superiorly there was persistent modified resonance, with variable respiratory sound, which, as repeatedly happened, was loud one day, and accompanied with coarse muco-crepitus amounting almost to gargonillement, and on the next was entirely suppressed, the patient expectorating fetid blood-stained pus, and the liver and heart displaced, the diagnosis of primary disease of the lung, probably

[a] Medico-Chirurg. Transact., Vol. xxvi.

malignant, with copious secondary liquid effusion into the pleura would, not improbably, be made.

The features in the group of physical signs just presented which possess special significance, and which, attentively considered, must suggest a different view of the case, are the displacement of the heart *upwards*, and of the liver *forwards*. Primary disease of the lung with consecutive serous effusion into the pleura, could, under no circumstances, effect displacement of these organs in the particular directions indicated.

It was manifest that the displacement of the heart must have been effected by pressure acting from *below*; this could not have been the result of pleural effusion, or of any known form of pulmonary disease. It must have been due to one of four causes, viz.:—

Enlargement of the liver.
Circumscribed perihepatic abscess.
Posterior mediastinal tumour.
Inferior thoracic aneurism.

There was no enlargement of the liver, as determined by palpation and percussion.

Of vertical displacement of the heart by an empyema of the right side, passing through the diaphragm, and limited by adhesion in front of the liver, I have submitted an example to the Medical Society in the course of the present session. But in the case now under consideration there was no disease on the right side of the chest; the liver was of normal consistence and volume, and hepatic dulness extended only to the sixth rib in the right mammary line. Irrespectively of the extreme rarity of tumour of the posterior mediastimum, of a size sufficient to cause displacement to the extent in which it existed here, the rapidity with which it was effected, and the absence of dysphagia, rendered such a view highly improbable. Aneurism at the base of the thorax alone remained to explain the phenomenon, and it was quite competent to explain it, as will be readily inferred from a consideration of the relative anatomical positions of the heart and the aorta in this situation.

Aortic aneurism will equally suffice to account for the forward displacement of the liver, and almost to the exclusion of other causes; this displacement was a prominent feature in Dr. Beatty's well known case; it likewise occurred in a case of aneurism of the hepatic artery reported by Dr. Stokes. But there was no evidence of abdominal aneurism of any form, or indeed of any kind

of abdominal disease here; the signs having reference exclusively to the thorax.

When to the evidence thus obtained we add persistent murmur at the xyphoid cartilage, having the precise rhythm already indicated, by which it was distinguished from all endo-cardial phenomena; loud bellows murmur along the lumbar spine; expectoration of pure florid blood; and excruciating pain of a shifting and intermittent character in the back, abdomen, and left lumbar region, coupled with the history already given, a correct diagnosis will seem all but inevitable.

M. A. Richet[a] says that the *bruit de soufflet* is never absent from aneurism, save when the orifice is so large that the blood meets no obstruction in entering or leaving the sac; and that it is better pronounced the larger the sac, and the narrower the orfice.

The case which I have narrated will serve as a commentary upon this statement, of which I will only further remark that it is manifestly not the result of personal observation and study.

M. Bertin,[b] speaking of spinal pulsation and sound as a sign of aneurism, says—"Aneurisms of the descending thoracic aorta, and especially those which erode the spinal column, will manifest their existence by single pulsations corresponding to the eroded vertebræ; a sign the more certain that, as M. Laennec has well remarked, the double contractions of the heart are rarely audible in the back."

In the foregoing statement, the truth of which will be readily admitted, murmur is not included, which in doubtful cases, heard along the spinal column, is undoubtedly of prime significance if not absolutely pathognomonic.

My friend Dr. William Moore in a recent communication to the Medical Society insisted upon the value of this sign. It is the more valuable because it may be regarded when present as capable, with the aid of posture, of establishing the differential diagnosis between solid tumour and aneurism.

In the case now submitted there was at least on one occasion, at an early period of its history, a distinctly double sound heard over the left back. At the same time, but not subsequently, a second murmur which was diastolic in rhythm was heard at intervals at the ensiform cartilage; and finally, towards the close of the case, a loud single and systolic *bruit de soufflet* was heard over the lumbar

[a] Nouveau Dictionnaire de Médecine et de Chirurgie Pratiques. Paris, 1865. Article Anévrysmes.

[b] Traité des Maladies du cœur et des Gros Vaisseaux, page 145.

vertebræ. At that time the aneurism had descended along the lumbar spine, and passed beneath the diaphragm as far as the left kidney.

This *bruit* was heard in the sitting posture; a circumstance of some value in regard to the observation of Sir Dominic Corrigan on the subject of cessation of aneurismal murmur in this part of the aorta, on the patient assuming the erect posture.[a]

Dr. Stokes has met with only one example of double pulsation and sound in abdominal aneurism. The case above given, which, however, was one of inferior thoracic, not abdominal aneurism, affords an example of this phenomenon in connexion with an adjacent portion of the aorta.

Since the publication of the case of the locksmith Lelong by M. Bertin,[b] observed by him in 1805, and reported in his 42nd observation; and more especially since Dr. Beatty's report[c] of the case of a gentleman who died of abdominal aneurism in 1829, acute and paroxysmal neuralgic pains in the back and abdomen have been regarded as diagnostic of this disease. These pains have been usually attributed to tension of the solar plexus; but in the case now reported the branches of this plexus must have escaped much pressure or tension, yet pain of the specific character mentioned existed. This may probably be accounted for by reference to the pressure which the lumbar plexus must have sustained.

I have already indicated my opinion, that the murmur heard beneath the right sterno-clavicular articulation during the last few weeks of the patient's life was due to the sharp curve which the aorta must have undergone in this situation, in consequence of the displacement of the heart upwards; the absence of aneurismal dilatation in this portion of the vessel was confidently affirmed at the time the observation was made.

The exaggerated impulse of the heart in its abnormal position was due manifestly to that of the aneurism which lay in close contact with it.

Death by exhaustion or asthenia in cases of large aortic aneurism has been found to have occurred in more than one-third of the cases collected by Dr. Crisp.

[a] Dublin Journal of Medical Science, Vol. ii.
[b] Opus citat, page 115.
[c] Dublin Hospital Reports, Vol. v.

ART. IV.—*Observations on Traumatic Fever and the Treatment of Wounds by the Antiseptic Method.* By ROBERT M'DONNELL, M.D., F.R.S.; one of the Surgeons to Dr. Steevens' Hospital.

THE fevers which come on in consequence of wounds more or less severe demand the most constant attention of the surgeon. To shun the dangers arising from them, possibly to prevent their occurrence altogether, is one of the most desired objects of the practitioner; to study with care the precise circumstances which give rise to them is one of the worthiest aims of science. Within comparatively recent days much has been done in each direction. The researches of Billroth, Weber, and others have tended to elucidate the causes of the phenomena of wound-fever; the practice introduced by Lister has tended to prevent its occurrence and disarm it of its dangers. To lay before my readers an analysis of the observations and experiments of the former, and some of my own experience as regards the latter, is my object in the following pages.

In the year 1702, Musitanus explained traumatic fever as being a commotion excited in the system by the absorption of the putrid matter of wounds; Roser more recently adopted the same view, and added many arguments of great value in support of an analogous theory of traumatic and septic fevers, so that by degrees many surgeons adapted their practice to this hypothesis of infection by putrescent substances. In December, 1866, Maisonneuve presented to the Academy of Sciences an exposition of the theory of surgical intoxications, in which, comparing traumatic fever with the other febrile affections consequent on wounds, he came to regard all these accidents as "the result of poisoning due to the introduction into the torrent of the circulation of toxic substances produced by the organism itself." I shall presently examine briefly some of the other theories put forth in explanation of the febrile symptoms following on wounds; for the present let me say that there is no theory with which the facts of clinical observation, as well as of physiological experiment, harmonize so well as the foregoing. According to it simple traumatic fever, secondary fever, and the still worse forms of putrid, purulent fever (pyemia, septico-pyemia, &c.), seem to be but different varieties of the same sort of infection; an infection or poison resulting from inflammation, suppuration, or decomposition, and generated within the organism itself. It must

follow as an obvious deduction from this theory that if we can prevent the occurrence of the inflammation, suppuration, or decomposition, which generates the poison, we prevent the occurrence of the fever.

SYMPTOMS, COURSE, AND TERMINATION OF SIMPLE TRAUMATIC FEVER.

First, what are the symptoms of simple uncomplicated traumatic fever? What course does it usually run? When, and how does it terminate? It seems strange that although in almost all our surgical text-books we find pages devoted to inflammatory and suppurative fever, we find no definite reply to these elementary questions. Perhaps they are best answered by giving a case in illustration.

James Andrews, aged seventeen years, was admitted to Dr. Steevens' Hospital, on May 12th, 1868, having at nine o'clock in the morning received an extensive scald on the back of left leg (1st, 2nd, and, to some slight extent, the third degree of Dupuytren). The scald was treated with linimentum calcis. The patient was well enough to leave hospital on June 1st.

The fever which accompanied this accident ran the following course:—

Day after Injury	Pulse	Respiration	Temperature		Observations
			Morning	Evening	
1st day,	120	30	102	102	Tongue red and dry.
2nd „	116	28	101	102⅘	Do. do.
3rd „	116	28	100⅗	102⅗	Tongue moist.
4th „	120	28	102	101⅘	
5th „	110	26	100⅗	100	Suppuration.
6th „	104	24	99	99⅗	
7th „	100	24	98⅘	98⅗	Suppuration profuse.
8th „	90	18	98⅗	98⅗	
9th „	84	18	98⅘	98⅘	

Billroth has observed upwards of a thousand such cases, and from them has established the ordinary types of traumatic fever as

met with in individuals who up to the moment of the accident have been in the enjoyment of their ordinary health.

A primary and important fact to establish is that fever is not a *necessary* consequence of every severe wound or operation. It may be absolutely wanting in some cases. Neither the seat nor the extent of the lesion enables one to predict the intensity or even the occurrence of the fever.

The appearance of the fever after operation or accidental wound is usually rapid; it appears to be exceptional for its onset to be announced by rigor. Possibly the cold stage may be merged in the period of depression or shock, although it is found that in cases where there has been considerable collapse the fever does not supervene early. As a rule the fever appears on the first or second day after the injury. In 87 per cent. of Billroth's cases within the first two days. The highest point of temperature or fastigium is generally reached on the first or second day; it ordinarily varies between 100° and 103° F., and only in rare cases mounts to 104,° a point which it very seldom passes. The mere fact of the temperature mounting to 104° Fahr. does not of itself lead to any important prognostication; provided it be transient this high temperature does not portend anything serious. It is a different matter if it persists; if it is prolonged beyond a couple of days it almost certainly indicates that mischief is brewing; either an extension of the inflammation or some new inflammation supervenes.

The subsidence of the fever commences after an augmentation of temperature; it goes on rapidly, sometimes without critical phenomena, by resolution, sometimes by a sort of crisis, diarrhea, &c., but sweating, augmentation of the urinary deposits, are rare as critical phenomena.

The increased temperature is the symptom of traumatic fever to which, as compared with other symptoms, most importance must be assigned; the frequency of the pulse, and acceleration of the respiration are usually proportioned to it. The nervous and gastric symptoms vary much in different individuals; they do not seem to be so much in relation with the fever itself as its complications.

The total duration of the traumatic fever may be said to be from two to seven days. When it is prolonged beyond seven days, we may dread the appearance of some complication; the fever then ceases to be simple traumatic fever; it is at least a new form called secondary fever.

The following case illustrates one of simple traumatic passing into secondary fever:—

Julia Doyle, aged thirty, admitted February 7th, left arm mangled by machinery; also large lacerated flap wound of abdomen; amputation above elbow; subsequent sloughing of flap on abdomen. Discharged on May 2nd.

Day after Injury	Pulse	Respiration	Temperature	Observations
1st day,	124	29	...	
2nd ,,	140	34	101°	
3rd ,,	140	34	102⅕	
4th ,,	140	32	101⅗	
5th ,,	120	30	100⅘	
6th ,,	120	28	99⅗	Tongue very dry.
7th ,,	110	25	101	Do. do.
8th ,,	112	25	100⅕	Do. do.
9th ,,	114	25	101	Do. do.
10th ,,	120	28	102	Flap on abdomen sloughing
11th ,,	120	28	102⅕	Tongue moist.
12th ,,	124	30	102⅕	
13th ,,	120	28	102⅕	
14th ,,	120	28	101	
15th ,,	116	28	102	
16th ,,	112	26	101	
17th ,,	112	26	101	Sloughs separated, leaving large suppurative surface
18th ,,	124	30	102⅗	
19th ,,	124	30	102⅕	
20th ,,	112	28	101⅘	
21st ,,	108	25	100	

The secondary fever, which is obviously identical with the suppurative fever of some authors, is, properly speaking, a not infrequent

complication of traumatic fever; it occurs in about one-third of the cases, and is apt to take place in subjects debilitated by severe nervous shock, by hemorrhage, or by copious suppuration. The probable causes of it are the retention of pus or of some products of decomposition; or perhaps the extension of inflammation from the wound to neighbouring parts. Simple retention of urine or of fecal matters may occasion some exacerbation of the fever.

At all events, this so-called secondary fever is an intermediate link connecting simple traumatic fever with the graver forms of purulent infection (septicemia, pyemia, &c.)

EXPERIMENTAL RESEARCHES.

Many experiments have been from time to time undertaken in the hope of solving some of the problems connected with purulent infection. The researches of Lee, Bennett, Ducrest, and Castelnau, Sédillot, and others, are well known. The experimental observations of Billroth and Weber have, in my judgment, greater value than any that have preceded them, and the closely similar results which they have obtained give great weight to their conclusions.

The memoir of Billroth has been translated almost *in extenso* in the *Archives Generales de Médecine for* 1865. The observations of Weber have appeared in the *Deutsche Klinik*. I shall attempt to give a brief *résumé* of their experiments, which are numerous, and have been performed with much care.

From Hunter's time up to the present, many observations have been made on the temperature of inflamed parts, and I may add that a good deal of difference of opinion has existed upon this subject. Indeed, Billroth and Weber were not themselves found to be exactly in accord upon this question.

Weber has, however, more recently repeated the beautiful experiments of Simon and Montgomery, made with the assistance of the thermo-electric apparatus and galvanometer, and has obtained results identical with theirs. These results also show a very remarkable degree of constancy, thus in thirty-six out of thirty-nine cases, the temperature of the inflamed parts was found to be elevated in some degree. Weber and Simon, therefore, coincide in establishing the following propositions:—

1st. Inflamed parts are warmer than the corresponding healthy parts.

2nd. The arterial blood going to inflamed parts is not so warm as these parts themselves.

3rd. The venous blood coming from inflamed parts is less warm than these parts, yet it is warmer than the arterial blood, and warmer also than the venous blood of the symmetrical healthy part.

Hence, it appears that wounds as centres of inflammation possess a temperature more elevated than healthy parts; that in fact there is going on in parts subject to inflammation, an active combustion or oxygenation, causing a slight increase of temperature of the blood; yet it is quite certain that this increase is not sufficient to account for the amount of heat developed as an accompaniment of traumatic fever. To explain this some other cause must be sought for.

Billroth and Weber have instituted a series of experiments, upon the effects produced by the introduction into the organism of a variety of substances such as the different products of suppuration, various septic substances, and blood. Some of these substances, when injected, either subcutaneously or into the veins, are found to excite inflammation either local or in the viscera; such are designated as *phlogogenic* substances or inflammation-exciters; whilst those, on the other hand, which cause an increase of temperature without exciting inflammatory action, are named *pyrogenic* substances or fever-exciters.

In a first series of experiments, Billroth injected beneath the skin or into the veins of dogs fresh or altered pus; these researches showed that the injection of such liquids always caused a constant increase of temperature, the highest degree being reached twenty-two, twenty-five, or twenty-eight hours after a simple injection. These purulent matters, when injected, likewise produce a local inflammation, generally giving rise to abcess, sometimes to gangrene of the skin.

In these experiments it is true that both local inflammation and fever resulted from the injections; yet the increase of temperature does not appear to be attributable to the local inflammation or to arise as a consequence of it, for the injection of the purulent matters into the blood gave rise to increase of temperature before any local inflammation was excited.

Billroth's next step was, in a second series of experiments, to try to determine which of the constituents of pus may be that to which we should really attribute the fever-exciting property. In order to decide this question he selected various substances found in pus undergoing decomposition; with these he made injections both

subcutaneously and into the veins. In this way he investigated the properties of sulphuretted hydrogen, bi-sulphuret of carbon, sulphuret of ammonium, carbonate of ammonia, leucine. As regards local effects the following results were obtained:—Sulphuretted hydrogen and bi-sulphuret of carbon do not give rise to any local inflammation; sulphuret of ammonium causes suppuration with fever; carbonate of ammonia produces very intense local effects followed by fever.

As regards temperature, sulphuretted hydrogen does not induce any change; bi-sulphuret of carbon caused a considerable augmentation of heat; sulphuret of ammonium produced no immediate effect on the temperature. But leucine in three experiments excited an increase of heat so considerable that the thermometer stood at 104° Fahr. Carbonate of ammonia gave very remarkable results, entirely different from all the other substances; injections of it were always followed by a lowering of temperature proportionate to the quantity injected.

This phenomenon is unquestionably one of great interest, especially when we consider its bearings on the symptoms of uremia.

As regards the constitutional disturbance which it excites, Billroth has found that perfectly fresh pus acts even more energetically than desiccated or putrid pus.

Weber has verified many of the foregoing results, and even confirmed many points still doubtful. Thus he has found that pus, the filtered serum of pus, sulphuretted hydrogen, and various products of exudation, are of a phlogogenic nature, that is, excite inflammation. Besides every one of these substances is likewise pyrogenic, that is, they excite a fever and cause an augmentation of temperature not dependent on the local inflammation. He has found also that not only carbonate of ammonia, but likewise butyric acid causes a remarkable lowering of temperature. Weber has also discovered the important fact that not only does the pus of one animal, when injected into the veins of a second, excite fever, but the pus of an animal when injected into its own veins is followed by an intense febrile excitement in this same animal. He has likewise shown that the blood itself, under certain conditions, may excite fever; in fact when the blood of a dog attacked with fever is injected into the veins of a healthy dog, this animal is attacked with fever. The blood bears with it the ingredients which excite the fever, and each fresh injection causes an exacerbation of the febrile condition.

Weber has not limited himself to the study of the temperature

and the phenomena of inflammation; he has likewise made observations on the variations of weight in his various experiments, and has brought to light some most interesting results; comparing the loss of weight of animals affected by fever in consequence of injections of putrescent matters, with the loss of weight resulting from simple inanition, he has established the important fact, that fever produces a loss of weight about twice as great as that due to absolute starvation. According to the experiments of Bidder and Schmidt (made in order to test the accuracy of Weber's results), a cat deprived of food lost, in the first six days, 2·97 per cent. of its own weight; a cat, in which fever had been excited by the injection of sulphuretted hydrogen, lost in two days 3·7 per cent. of its own weight, that is to say, 0·73 per cent. more than in the case of simple inanition. A dog, into whose pleural cavity pus had been injected, lost 2·8 per cent. of his own weight every day; whilst Bischoff and Voit have observed that a healthy dog kept fasting for six days lost only 1·28 per cent. of his own weight daily. It is clear, therefore, that the loss of weight occurring as a result of fever is not a mere consequence of loss of appetite and diminished nutrition, but a consequence of the augmented process of combustion excited by the ferment which, acting on the blood, has called the fever into existence.

Dr. R. Schneider,[a] of Königsberg, has lately published some very valuable observations on this subject, made upon patients in his surgical clinique. His facts are derived from a comparison of patients affected with traumatic fever with persons not suffering from fever; the latter class of cases were patients who happened to be in hospital, but on account of accidents or complaints unaccompanied by traumatic fever. He admits that, in spite of every trouble and care which he could take, his results are not absolutely free from error; indeed it is obvious that where, as in some cases, the entire bed and bedding had to be weighed along with the patient, exact scientific accuracy could hardly be obtained. Yet it is clear from his tables that he took great pains to obtain as correct results as possible. His general conclusions are:—

1st. The results of those researches which are purely physiological (that is those made on individuals free from fever), agree with the results of other observers, and show that in the human economy there is no absolutely constant relation each day between the income

[a] Untersuchungen über das Körpergewicht während des Wundfiebers: von Dr. R. Schneider: Archiv für Klinische Chirurgie von Langenbeck. 1869. P. 131.

and expenditure; yet that these on the whole bear a fixed relation to each other, although varying in different individuals.

2nd. During one day's complete abstinence from food no real difference was observed in the materials given off.

3rd. A diminution of weight takes place in consequence of traumatic fever. This does not always advance steadily and at equal rate to the end of the fever, but during the progress of the illness, particularly towards the later period of it, is subject to slight variations. After the fever subsides the weight commences to increase, and if it has not been of an intense kind the original weight of the body may be attained in one week. A longer time is needed if the fever has been intense, or of long duration.

4th. If secondary fever comes on, the weight of the body falls off again, even although it previously began to increase.

5th. In patients suffering from traumatic fever as well as in those free from fever, there are daily variations; as a rule the minimum weight is observed in the forenoon and the maximum after supper.

6th. In case of traumatic the loss of weight is usually greatest on the first day. It amounts to $\frac{3}{4}$, or 1 or even 2 per cent. of the weight of the body, when there has not been any considerable loss of blood either at or after the operation; the loss of weight is more marked if there has been much hemorrhage; if the fever runs high in consequence of any complication, such, for instance, as erysipelas, the loss may amount to 3 per cent.

7th. In consequence of traumatic fever, there is a less quantity of nutriment taken into the economy, but likewise the outgoings are diminished. As regards the latter there is a striking difference between those which may be called the perceptible and the imperceptible; thus, whilst the first (especially the quantity of the urine) diminish absolutely as well as relatively, the perspiration undergoes a considerable augmentation.

8th. In pyemia a great loss of weight takes place, from 3 to 4 per cent. of the weight of the body daily.

9th. The percentage of urea in the urine keeps pace with the progress of the fever: the largest elimination of this material follows some time after the highest degree of temperature. A more violent degree of fever does not usually cause a more marked elimination of urea, but through individual influences a large elimination of this substance may take place even when the fever is slight.

The theory that the various phenomena observed in the fever

following surgical operations are due to the so-called apocoptic plethora (that is, to the condition of plethora due to the relative augmentation of the mass of the blood in comparison with the weight of the body, which is lessened by the sudden removal of an important portion or a limb), is not now generally accepted. Nevertheless an observation made by Weber has some value, inasmuch as it seems to be a positive refutation of any such idea, proving, in fact, that no such plethora actually occurs. Before performing an operation upon a dog Weber measured with a manometer the tension of the circulation. Before the operation the tension, as measured in the carotid, oscillated between 33 and 71 millimetres. The right femoral artery was then tied; the pressure oscillated between 35 and 73. During amputation of the right thigh it was not higher than between 30 and 40. When the remaining arteries were tied the pressure was from 44 to 53, and then gradually became fixed at between 34 and 48. Thus, *before the operation*, the mean of the oscillations was about 38 millimetres, with a maximum of tension of 73 millimetres; *after the operation* the mean of the oscillations was not more than 14, with a maximum tension of 48 millimetres.

Much discussion has taken place upon the part played by the nerves and their irritation in the increase of temperature, which is so prominent a feature in traumatic fever and traumatic reaction. It is a subject well deserving of further research. We know that the temperature of the ear and side of the head is augmented after division of the cervical sympathetic nerves: in this case more blood circulates in the part in consequence of the paralysis of the muscular coats of the blood vessels; and although comparatively hotter than the symmetrical portion of the body, yet the temperature does not exceed that which is the normal heat of the blood in internal parts. In some injuries of the spine, however, the case is different. Sir B. Brodie has recorded a case of fracture of the spine in which the temperature rose to 111° F., when the bulb of the thermometer was placed between the scrotum and the thigh. A case has been lately reported by my colleague, Dr. Edward Hamilton, in which the temperature was found to be 106° in the axilla; in another instance, in a patient of my own in Dr. Steevens' Hospital, the thermometer stood at 104° in the axilla. Here there is an absolute increase in the temperature of the blood running far above the normal temperature of that fluid in any part of the body; and the question naturally arises, to what is this increase owing? Is it due

to an irritation of the nerves arising from the part of the spinal cord engaged in the injury? or is it a secondary effect,—the nervous irritation causing, in the first instance, a disturbance in the functions of some organ (as suppose the liver) which, when thus excited, secretes some morbid material, and this acting through the blood as a phlogogenic matter produces fever heat? These are questions which science has still to answer. One problem in connexion with this interesting subject has been solved by the experiments of Breuer and Chroback.[a] They proposed to themselves to investigate how far it was possible to excite traumatic fever by injuries or operations upon parts in which all nervous communication was interrupted by severance of the nerves. The experiments were made upon dogs; the sciatic, obturator, and crural nerves were cut across at their exit from the pelvis, and portions of them resected. The filaments of the sympathetic were also divided, so far as was possible. The leg and foot were thus deprived of communication with the nervous centres. The wounds were allowed to heal, and the further experiments were not undertaken for from four to eight weeks later. Wounds were then inflicted upon the enervated parts of these animals, and, for the sake of accurate comparison, traumatic fever was likewise excited upon healthy animals by the infliction of wounds as nearly as possible similar. There was a close analogy in the results obtained upon these two sets of animals. In the case of the paralysed dogs the temperature rose upon the second day after the injury to over 105° F.

From this series of experiments MM. Breuer and Chroback have been led to the conclusions that the fever accompanying traumatic inflammations is independent of the action of the nervous system.

Dr. B. W. Foster has drawn attention to the fact that in diabetes the temperature is always below the natural standard, the depression usually varying from one to three degrees. In cases of recent origin the temperature ranges between 96° and 97·5° F., never mounting to or going beyond the normal standard, except from some disturbing cause. In more advanced cases the temperature is nearly always below 97°, and occasionally falls as low as 94·5°. Whether this depression of temperature may be due to the saccharine matter circulating in the blood is a question concerning which I am not aware that any precise experiments have been made.

[a] Zur Lehre von Wundfieber.

I have myself observed that after lesions of the medulla oblongata, made in order to induce artificial diabetes, there follows a marked depression of temperature. The normal temperature observed when the bulb of the thermometer is introduced some inches into the rectum of a rabbit is 104° to 105°; after puncturing the medulla oblongata (or spinal cord high up) the temperature falls sometimes to 96° or 97°. How far this lowering of temperature is to be regarded as the converse of the increase observed in some other injuries of the spine, is also a subject for further investigation. I would merely observe that there is nothing contradictory in supposing that injuries of different parts of the nervous centres may thus secondarily be followed, one by increase, another by diminution of temperature; a disturbance of the functions of the lung, liver, or other organ, being followed in the one case by the formation of some material, which, passing into the circulation, acts as a fever exciter, as sulphuret of carbon, or in another as a heat depresser, as carbonate of ammonia.

Without discussing further the various theories, mechanical, zymotic, septic, &c., which have each had able men among their advocates, let me say that the septic theory is that which is at present in the ascendant, as being that which gives the simplest explanation of the symptoms of traumatic fever and purulent infection: it is that which has given birth to the antiseptic method of treating wounds, or that known as Lister's method.

A considerable amount of evidence has now been accumulated from various practitioners upon the subject of the treatment of wounds by Lister's method. Although, as regards this as upon most similar topics, there is some diversity of opinion, yet it must, upon the whole, be singularly gratifying to the distinguished surgeon who has done so much to establish the antiseptic treatment of wounds to find how strong the testimony of independent observers is in favour of his practice. A valuable contribution upon the subject has already appeared in the pages of this journal. It would, I conceive, be tiresome and unnecessary to enter fully into the details of cases in which I have myself had experience of it. The success which I have witnessed as attendant upon it leads me to form a very high estimate of its merits. Indeed my opinion, as formed from the results which I have met with, may be expressed in very few words. It is briefly this: That the torsion of arteries combined with antiseptic dressing carefully applied, and with due attention to details, leaves little to be desired as regards the treat-

ment of wounds. This department of surgery would seem almost to have attained by these means the highest degree of perfection of which it is capable.

Under its influence the wound itself heals rapidly, kindly, and with little or no suppuration; while the traumatic fever is prevented. I have now watched a large number of cases of formidable operations, removal of tumours, amputation, &c., in which the temperature was accurately noted from day to day, yet the thermometer was rarely found to rise above 100°.

It is, of course, true that in order to establish the fact that antiseptic dressings containing carbolic acid prevent traumatic fever and its more formidable allies from attacking patients after operations and injuries, we must have comparative results from similar cases treated upon different plans. Yet, of this I am convinced, that since I have adopted the use of carbolic acid dressings, I have not met with constitutional disturbance among my cases such as I was used to formerly.

It is quite certain that many cases of even formidable wounds may recover without any traumatic fever supervening without the application of any antiseptic dressing; the question is, are not these cases greatly increased in number by this treatment? I cannot hesitate to say that they are. Billroth met with twenty cases out of 273, in which the traumatic fever was absent. Among these were some grave operations upon the face, one partial resection of the upper jaw, lesions of the phalanges, &c. Mr. Thomas Pick, in an interesting paper in *St. George's Hospital Reports*, states that in 108 cases tabulated by him thirty-five were absolutely free from symptoms of a febrile nature. Among these thirty-five cases several were wounds of a very severe description, including among others an amputation of the leg, two compound fractures, and an amputation of the breast; whereas several wounds of a slight description were followed by very severe symptoms.

Under carefully applied antiseptic treatment this proportion seems to be reversed: those cases are few in number in which any very marked symptoms of traumatic fever occur. It is true some increase of temperature usually takes place on the third or fourth day, yet very rarely have I seen formidable symptoms arise, and I believe I am correct in stating that no case of pyemia has occurred in Steevens' Hospital in any case hitherto in which carbolic acid dressing has been used.

My resident pupils, Messrs. Fitzgerald and Rathborne, have

noted the temperature, &c., in a large number of cases, from which I select the following, in all of which carbolic acid dressing was used:—

1st. Under the care of Dr. Colles—J. Middleton, aged forty-seven. Operation: Removal of a large fatty tumour from left breast (colloid lipoma); patient in good health before operation; chloroform used. Operation February 15th, 1868.

Day after Injury	Pulse	Respiration	Temperature
Day of Operation,	60	14	98⅘
1st day after,	60	14	99
2nd „	72	16	101
3rd „	72	16	100⅗
4th „	70	16	98
5th „	72	16	98
6th „	68	14	98
7th „	68	14	98

2nd. Under the care of Dr. Hamilton—Mrs. Kane. Operation July 25th, 1868: Removal of malignant tumour of left breast; chloroform administered; torsion used to control hemorrhage; wound dressed with carbolic oil and paste; no suppuration; wound healed by first intention:—

	Temperature.
Day of Operation,	98·4
1st day succeeding,	98·4
3rd „	99
4th „	98·4
5th „	99
6th „	98
7th „	98

3rd Case: Under my own care—J. Leedom, aged fifty. September 12th, 1868: Amputation of right leg for disease, consequent upon compound dislocation of ankle-joint; torsion applied to all

vessels save the anterior tibial artery, which was controlled by acupressure.

On the day succeeding the operation:—

Pulse.	Respiration.	Temperature.
100	30	99·4

Upon no day during speedy convalescence did the constitutional disturbance exceed this.

I might add to these, did I not feel it to be unnecessary, a large number of similar cases. It would be only to enumerate such cases as have already appeared in the pages of this journal in the communication of Dr. W. MacCormac, of Belfast, on the antiseptic treatment of wounds. He states that "he has observed over and over again the almost total absence of pain, inflammatory swelling, and surgical fever, where such might otherwise have been expected to occur." To this I can merely add that in the cases tabulated by me the absence of fever has been proved by thermometric observations.

Let me say in conclusion that I heartily concur with Dr. MacCormac in deprecating the attempts which have been made to deprive Professor Lister of the immense merit of having introduced this plan of treatment, a plan which bids fair to achieve great results, a plan which I doubt not will gain a lasting reputation for its author.

ART. V.—*A Case in which Septic Infection, which terminated in Death, was caused by the Breaking of a Dracunculus or Guinea-Worm, during an attempt made by the Patient to extract it.* By JOLLIFFE TUFNELL, F. and Examiner in Surgery R.C.S.I., M.R.I.A.; Surgeon to the City of Dublin Hospital; Ex-Regius Professor of Military Surgery.

THE gentleman the subject of this communication was thirty-eight years of age, a temperate and very healthy man. He had been in India for some years engaged upon the geological survey of the Madras Presidency, and, as such, was necessarily much exposed to jungle, and other influences tending to fever, and he suffered accordingly from ague on several occasions. He also had guinea-worm a few years back, which attacked him in an unusual situation, namely, above the condyle of the humerus of the left arm.

This being treated by a native in the usual manner, and rolled out from day to day, was extracted without suffering or much annoyance.

He arrived in Dublin from India upon the 16th of December, 1868. Upon the 18th his left foot felt tender; so much so, indeed, that he could not put on his boot, and had to go about in a slipper. By so doing he got relief, and then resumed the boot; but the pain returned again, and thus he went on, at one time worse and another better, until the 18th of February, never suspecting the real cause of the mischief.

Upon this day the guinea-worm showed itself in the customary manner as a small boil, from the centre of which the head of the creature protruded. Recognizing it he at once proceeded to extract it, and meeting with no resistance for a considerable while imagined that he could draw it out entire. Unfortunately, his confidence was misplaced; the worm separated in its continuity, and retreated beneath the integument, when about eighteen inches had been gathered into the patient's hand.

The orifice through which this portion had been extracted was situated behind the inner malleolus of the left foot, midway between it and the Achilles tendon. The worm lay in the areolar tissue adjacent, with its tail in all probability coiled up around the tendon itself, because in the progress of the case phlegmonous inflammation showed itself upon the outer side equally with the inner and counter openings, for the escape of pus had to be made there.

A few hours after the breaking of the worm acute inflammation supervened, and extended to the knee. This, however, did not cause any alarm to the patient. When called in upon the 20th I found him sitting in the parlour smoking a cigar, and he seemed to be quite annoyed at being ordered up to bed. Linen cloths soaked in Goulard's lotion were directed to be applied to the limb, which was well raised upon a pillow, and the most perfect quietude enjoined.

All went on well for a time, the local inflammation of the leg subsiding, and the orifice of the worm's exit looking healthy, pressure by a sponge from above downwards being unattended by pain, and discharging daily through the orifice a small quantity of putty-like pus. Upon the morning of the 4th of March, however, that is, sixteen days from the breaking of the worm, all was changed. An intense rigor now suddenly set in, as if the

intervening period had been one merely of incubation, and the mischief had by this become developed. Having suffered from intermittent fever this rigor was looked upon by the patient himself as merely an attack of ague, and was expected so to have passed off, but no sweating stage succeeded. Violent fever supervened, with gastric irritation, as if the stomach, in sympathy with the nervous system, had in its great centre, in the solar plexus, primarily yielded, and for forty-six hours nothing could be retained. Counter-irritation by mustard was resorted to, and only Wenham ice given; even it for a while was not retained.

At the expiration of this time the fever, restlessness, and irritability of the stomach disappeared, and improvement took place, but only for a few hours. At midnight of the 6th inflammation of the kidneys set in, with intense pain all round the lumbar region, and with suppression of urine. Dry cupping was now resorted to, and Dover's powder and gray powder freely given to act upon the liver and skin, with chlorate of potash as a drink. On the succeeding morning tympanitis had supervened, the whole abdomen being now distended with air. Turpentine fomentations were freely applied, affording some, but slight, relief. Broncho pneumonia of the left lung next followed, succeeded by pleurisy of the same side. These showed themselves upon the 9th, commencing above and travelling downwards until the whole organ and its covering were engaged. These diseases having established themselves there, and taking entire possession, as it were, of one side, passed over to the other, invading and seizing upon the right lung and pleura. With these profuse perspiration, exhaustion, and weakness, and rapidity of pulse, were the accompaniments. Quinine in doses of three grains, with an equal quantity of gray powder, were now given every third hour, and the lateral and posterior portions of the chest fully vesicated by cantharides ointment, and subsequently enveloped in raw cotton, oiled silk being applied over all. Constipation had existed from the commencement. This condition was attempted to be met by turpentine enemata, which not acting castor oil and turpentine in doses of three drachms each were given by the mouth. Under the combined influence the bowels yielded, and much yellow feculent matter of gruelly consistence was discharged. The stomach had now become retentive, and support was freely given in the shape of wine, brandy, essence of beef, eggs, jelly, &c. All sleep was absent, unless procured through the medium of narcotics. Three

doses of black drop (prepared by Mr. Evans, of Dawson-street), amounting to sixty-five drops in the whole, equal to 195 drops of laudanum, were required during the night to produce any rest. The pericardium now became engaged, effusion taking place. This, in eight and forty hours, had become absorbed, the dulness disappearing, and friction sounds becoming audible again. The dyspnea and distress of breathing, however, were excessive, the patient sitting propped up in bed, speaking in gasps, with the perspiration standing out upon the forehead in large drops, and exuding again as quickly as wiped off. Sudamina, too, existed over the whole body and upper half of the thighs.

The remaining portion of the guinea-worm now showed itself, having been pressed down in the elimination of the discharge, and its extremity having been seized with forceps it was drawn out and rolled up by degrees, the whole, to the extent of about eight inches, being finally detached, without, however, any relief to the constitutional affection being afforded. Matter having now manifested itself upon the outer side of the tendo Achilles this was cut into, and freely opened. Healthy-looking pus escaped, and a probe passing from one side to the other showed that this collection was in connexion with the original disease.

The urine during this time was dark and loaded with lithates; highly ammoniacal, and very fetid. The cough throughout was hacking and distressing, the efforts to clear the lungs of sputa exhausting the remaining strength in vain efforts to disengage the viscid mucous from the throat. Towards this, the latter period of the illness, the pectoral discharges too were frequently composed of small portions of clotted blood. Throughout the whole period—that is to say, from the occurrence of the rigour, despondency and weakness were extreme, whilst occasional wandering and delirium took the place of sleep. After struggling on for six-and-twenty days, at several times making a rally, and seeming as if about to convalesce, the poor fellow was attacked upon the 29th of March with acute pain in the diaphragm, followed by rapid breathing and bronchitic effusion, and he died upon the morning of the 30th.

No *post mortem* examination was made, the circumstances under which the patient died, with female relatives only in the house, forbidding the proposition. It is impossible, therefore, to say whether purulent deposits had taken place. None were indicated as existing in the joints, but in the lungs, in all probability, such would have been exposed, had these organs been incised. From the dyspnea

and dulness existing before death, I consider that they would also have been found œdematous to a very great degree.

I have named this case as one of septic infection, an animal poison having been introduced into the system, through the breaking of the worm poisoning the whole economy through decomposition of the blood. There was no evidence of phlebitic inflammation to be observed in the course of the vessels of the extremities primarily engaged. The debility that accompanied the disease from its onset, however, was such as would result from the circulation of pus in the system, or a sanious fluid arising from the decomposition of pus.

If then we take the symptoms that usually attend upon cases of pyemia or suppurative fever—viz., great depression and foreboding, with moaning, profuse perspiration and sudamina, broncho-pneumonia, pleurisy, pericarditis, flatulence, and abdominal tympany; constipation, sleeplessness, and hallucinations; rapid pulse, and rapid respiration, with bloody sputa; all succeeding with more or less rapidity upon intense rigor, and terminating in death, as characteristic, we have them here, and we cannot but regard this case as one and identically the same disease, and following upon a local cause, the rupture and decomposition of the worm. There was nothing in the constitution of the patient to predispose him to pyemic infection, or in the locality or surrounding circumstances to render him liable to such disorder. He was a healthy, temperate individual, even tempered and cheerful, and a good companion; the last man likely to generate a materius morbi in himself, whilst the house in which he resided was one of the most cleanly and healthy of any in the suburbs of Dublin. During the progress of the case I had the assistance of Dr. Stokes almost daily in consultation, and I believe I may fairly state that nothing that human means or human foresight could suggest to avert death was left untried by us.

With reference to the subject of guinea-worm, and its treatment by gradual extraction, I wish, in conclusion, to make one or two observations, for dracunculus or guinea-worm is really a formidable disease. Formidable under misadventure, such as in the instance just detailed, and formidable under ordinary circumstances, in its being general over a large portion of the tropics, and when attacking any individual incapacitating him for weeks, and even months, from taking any active exercise, in consequence of the risk which is incurred from rupturing the worm. From the first drawing forth

of the head until the final rolling out of the extremity he is in jeopardy, and, in the case of military (and cavalry soldiers in particular), totally unfitted for duty.

Any plan, therefore, that is found upon experience to be capable of hastening the elimination of the parasite, instead of causing risk to the patient, is deserving of attention, and I would, therefore, firstly, refer to the method employed by Sir Ranald Martin, and, secondly, to that by carbolic acid—the one being capable of adoption where the other might not be at hand.

Sir Ranald keeps up a constant stream of cold water, or, if procurable, of ice cold water, poured over the animal at each effort of extraction; and he finds that by numbing the creature it is much more readily drawn out than if left with its power of resistance unimpaired. Still under this treatment the guinea-worm cannot be expected to be withdrawn under a few days.

By whom the carbolic acid treatment was first adopted I know not, but in the month of December, 1867, it was employed by a medical missionary in Bengal, who writes in the *Indian Medical Gazette* for January, 1868, as follows:—

"A month ago, as I was halting for a few hours at the town of Gurhshánkar, in the Hoshyarpoor District, the arrival of a Doctor Sahib was quickly noised abroad. In the midst of a crowd which flocked towards the tree under which I was sitting, came some one staggering under the weight of a full-grown man, shown by his uniform to be a policeman. I soon learnt that the cause of his lameness was a guinea-worm, from the presence of which he had been suffering for several weeks. The outer end was, as usual, wrapped tightly around a stick, and protruded from the skin just below the right knee, on the outer side of the leg. A probe penetrated easily two or three inches upwards and inwards, after which the channel became too torturous to be followed, but could, by the patient himself, through the medium of sensation, be distinctly traced deep into the muscular tissues of the thigh. From the orifice, which was small, exuded a constant flow of somewhat ichorous pus. The worm itself was flat, like a slender tape, or strip of parchment, perhaps half a line in breadth: very thin, but seemingly tough. Every day the patient (whose name I forgot to record) had, by gently twisting the stick, succeeded in drawing forth an inch or two of the worm. The portion already on the stick was dry and black, and to all appearance utterly devoid of vitality; yet the patient dreaded nothing so much as that the worm

might break. He had previously, he told me, enjoyed excellent health; yet at this time he looked extremely haggard and wretched. The pain, though not very acute, was constant and most irritating, robbing the patient of his rest, and inducing a sympathetic fever at night. He would allow no one but himself to make traction by twisting the stick. In short, had I not seen this case, I could not have believed that a cause, apparently so trifling, could produce such severe constitutional disturbance. Certainly this man's sufferings were very great; and what was even more important, he was a perfect cripple. This last, perhaps, was on account of the knee-joint being somewhat involved.

"The *treatment* was very simple, and apparently successful. I happened to have a phial of carbolic acid, which never fails to give to 'low-lived' organisms their quietus. The *narvé* reaped the benefit of that circumstance. A pledget of cotton wool, secured by a thread to the blunt end of a probe, was dipped into the pure acid, and pushed gently up the sinus as far as it would go, the soft silver being slightly bent to favour its insinuation. After a few moments the patient was asked to make traction. He now found, to his delight, that the hold of the worm was much looser, and he drew out an inch or more with very little difficulty. Beyond this he could not get. The probe was re-introduced with a fresh supply of acid, and again he succeeded in drawing forth more than an inch. This process was repeated a third time with a like result. I was anxious to give the man a supply of carbolic acid, with a view to his repeating for himself this operation so long as there should be any need of it; but as he could not, or would not, furnish a phial, and I had none then to spare, I merely extemporized some 'carbolic acid ointment,' telling him to keep this applied to the wound.

"On returning to Gurhshánkar, less than a fortnight afterwards, I was very much pleased to find the self-same policeman standing on guard at the gate of the thanna, looking perfectly well and happy. His leg was free from all pain, and a fresh cicatrix marked the site of the orifice from which the *narvé* had protruded. He told me that he had kept the ointment applied. Each day he had succeeded in withdrawing many inches of the worm, and that finally, on the fifth day from the application of the pure carbolic acid, the last of the worm had been drawn out, 'making in the four days,' as he believed, 'a total of eighteen or twenty inches.' He confessed that within this time he had, by the advice of one of his Punjabi friends, employed divers fomentations and frictions, *in*

addition to antiseptic ointment; but he did not himself think that these measures had had much to do with his cure. Nevertheless, as this incident furnishes a crevice for the admission of error, it is but right that it should be mentioned.

"In conclusion, I would remark that, if I have such a case again, I shall, if possible, persist in the process of introducing the acid and making traction alternately, until the entire worm is removed. A patient might easily do it for himself. The *pure* acid should be used. In the above case the caustic, as well as the antiseptic, action of the pure acid seems to have favoured a speedy healing of the sinus,[a] which might otherwise have continued to suppurate *long after the exit of the worm.* Again, weeks of suffering had in this case elapsed since the worm had made its appearance on the outer surface of the skin. An army surgeon, by applying the acid the moment the worm appears, may reduce the patient's sufferings to the period preceding its protrusion. In case carbolic acid be not at hand, I would suggest the use of kamila, both externally as a poultice and internally in small doses, kept up for some time. We all know how it loosens the grip of tænia; and Dr. Hjalten, of Iceland, has shown very recently, in the *Edinburgh Medical Journal*, that a persistent exhibition of the tincture internally poisons (and so cures) certain abdominal hydatid cysts."

In the case just recorded, five days were passed in the extraction of but a portion of the worm!

A more rapid case, and one that crowns the treatment by carbolic acid, has lately been reported by Dr. Costello, of the Indian service.[b]

The mode adopted by the native dresser, under whose care the patient was, was the passing of a silver probe, dipped in a mixture of one part of carbolic acid to three of oil, up on each side of the worm as far as it would go along the sinus; then to roll up very gently so much of the animal as would come forth *freely*, renewing the application of the oil directly that traction became apparent; and so proceeding little by little until, at the end of two hours, the worm, measuring thirty inches in length, was drawn out entire.

[a] Somewhat as in Professor Lister's case of psoas abscess.—See The Lancet of July 27th, 1867.

[b] Through Dr. Costello's kindness I was enabled to exhibit to the Medical Society of the College of Physicians the worm, withdrawn in the brief period of two hours.

ART. VI.—*On Certain Forms of Paralysis and their Treatment by Electricity.* By WALTER G. SMITH, M.B., Fellow of the College of Physicians; Assistant Physician to the Adelaide Hospital.

ABOUT the latter half of the eighteenth and the early portion of the present century the virtues of electricity as a remedial agent were extravagantly lauded, and properties attributed to it which it is no longer acknowledged to possess.

The early volumes of the *Philosophical Transactions*, and the contemporaneous publications contain the most marvellous accounts of the wonders which electricity effected; for example, how Signor Donadoni, bishop of Sebenico, when seventy-five years of age, was cured of an old and severe attack of gout, by which he was hopelessly crippled, on being electrified for two minutes with a cylinder that was filled with discutient medicines. These medicated tubes or cylinders, introduced by Signor Pivati, were much recommended in 1747–48 in Italy, and by Winkler at Leipsic; and these writers imagined that not only odorous substances, such as Peruvian balsam, but even cathartics, sudorifics, diuretics, &c., would transpire through the pores of the glass, and transmit with undiminished force their medicinal virtues to the conductor of the machine, and to all persons in contact with it.

Pivati was warmly commended by Morgagni, and encouraged by him to proceed in his experiments. One of his patients was electrified with a cylinder containing balsam of Peru, and the man sweated and felt other effects, and when he had refreshed himself with sleep, he combed his head and found the balsam to have penetrated his hair, so that the very comb was perfumed. In like manner with sulphur and cinnamon.[a]

We are not at present in so much danger of according electricity too large a share of praise, as of allowing a general scepticism to unduly bias our opinion as to the real value of any remedy. It is scarcely to be thought that anyone who gives electricity a sufficiently extended trial, will ignore its beneficial action, or fail to be convinced of its utility in properly selected cases; but we cannot be too mindful that to form a correct judgment in this respect, close attention must

[a] A complete and able summary of the history and uses of medical electricity down to the middle of this century, is given by Mr. Donovan in the Dublin Quarterly Journal for 1846–47.

be paid to the form of electricity adopted, and the manner in which it is applied, taking into account also the dose, so to speak, and the length of time during which its action is continued.

We could not, for instance, expect benefit to accrue in a case of local atrophic paralysis of the hand, when the patient is directed to grasp two *dry* brass handles, and powerful induction shocks are transmitted for half an hour or an hour at a time, exhausting the excitability of the diseased parts, and unnecessarily stimulating a large tract of the body. Nor could anything but injury result from the prolonged application of strong induced currents to the enfeebled muscles of an old man, the sitting extending over an hour or an hour and a half at a time, and repeated in the evening. These are but instances of what there is little doubt is a common occurrence, and by which discredit is brought upon a valuable remedy.

It is clear, then, that if we look for a favourable issue from the use of electricity it must be employed advisedly and with proper attention to detail, and it is largely to neglect in this latter respect that we are to attribute the suspicion which has been thrown upon this agent, and the indifference with which it is regarded by many sober-minded physicians.

The principal causes which have tended to the comparative neglect of electricity, or have been a bar to the increase of our knowledge, are briefly these:—

1. Imperfect physiological knowledge. Neuropathology and physiology, especially in their relations to electricity, have advanced rapidly of late years; still we are far from having a sufficiently wide basis on which to ground a perfectly successful clinical application of electricity, and in some cases we must rest satisfied with the attainment of results of practical utility, without being in a position to explain them satisfactorily.

Accurate clinical observation, guided by the ascertained facts of electro-physiology, will prove the fountain head of future progress in electro-therapeutics, as it has been the foundation of our present knowledge, and it is to be hoped that, as the number of honest workers in this field increases year by year, we may ere long look for a solution of the difficulties which still invest some parts of the subject.

2. It is frequently not had recourse to, till all other means have been tried and found wanting, and accordingly the electric treatment is commenced under the most disadvantageous conditions, and medical men form their opinion of it after dilettanti trials, by which

blame is cast upon the means when it is the method that is at fault.

3. Indiscriminate use in unsuitable cases.

4. The incomplete and inaccurate mode in which the notes of cases are recorded, so that they are nearly useless for future comparison or reference.

5. Its application too often falls into the hands of ignorant or designing persons, who possess the instruments without the knowledge of how to utilize them with precision. Many of the earlier writers on electricity were opticians, whose object was naturally to promote the sale of the different articles of apparatus; in modern times, unfortunately, the spirit of trade is occupied not so much with the instruments as with the public on whom they are to be applied.

Then also, as now, churchmen were addicted to dabbling in medicine, and the celebrated Mr. Wesley was long engaged in the practice of medical electricity for the benefit of persons in his connexion, and he observed that he had scarcely known an instance in which shocks all over the body (!) failed to cure a tertian or quartan ague.

6. The misrepresentations of early writers, whose marvellous relations obscured the grains of truth that lay beneath their exaggerated stories.

7. The vain pretensions of many of its later advocates, and since it cannot accomplish all that its boasting supporters have asserted, it is, in spite of its powers for good, in danger of relapsing into the hands of charlatans.

Now, in urging these considerations, I would not be thought to recommend electricity in the spirit of a specialist as an infallible cure, or to claim for it extraordinary sanatory powers. Even in apparently suitable cases it will sometimes disappoint our expectations, and this fate it only shares in common with many of our most trusted resources, such as opium or belladonna. It is not, however, to be forgotten that a marked advantage of electricity as a means of combating disease is, that it interferes in no way with the action of other remedies, and, as a rule, its employment may go hand-in-hand with that of any general or special line of treatment which may be thought necessary.

It would far exceed the limits of a short paper to enter upon the discussion of even the principal beneficial results which electricity may be expected to fulfil in medicine, and I shall therefore restrict my observations to the consideration of the restoration of contractility in paralysed muscles, leaving aside for the present any notice of its

effects in re-establishing sensibility, in reducing abnormal contractility, or diminishing hyperesthesia, for under these four heads are comprised its real and definite uses to the physician. Paralytic affections appropriately engage our attention first, when we recollect that it was in great measure owing to its successful application in the treatment of these affections that electricity was enabled to gain a permanent place among the resources of therapeutics.

The cases I am about to detail may be grouped into the following classes:—1st, Facial palsy; 2nd, Paralysis from the poison of lead; 3rd, Paralysis from local injury or pressure, whether it be on the nerve or directly upon the muscle; and, 4th, Rheumatic paralysis of the limbs.

Facial Paralysis.—It will be understood that electricity is not indicated in all cases of palsy of the facial muscles, and can only be useless, if not mischievous, in severe traumatic cases. For example, a boy (J. Dean), aged five years, a few days before admission into the Adelaide Hospital, under Dr. Barton (March, 1868), had been accidentally thrown carelessly over a soldier's shoulder, and had fallen on his head. He lost a great deal of blood out of the left ear, became feverish and cross, and had several attacks of convulsions at noon each day. Paralysis of the left side of the face gradually supervened, the sense of smell was impaired, as also that of taste on the left side of the tongue. The mouth was considerably distorted, and he was quite unable to close the eye. It was manifest that the paralysis here was beyond the reach of electricity, and a trial on two occasions with the primary or secondary current failed to elicit any decided contractions, except in the lower fibres of the orbicularis palpebrarum.

Electricity is likewise contra-indicated when the paralysis is due to tumours in the cranial cavity, or in the parotid region involving the seventh nerve, or to disease of the temporal bone.

In almost all cases of rheumatic facial paralysis the electro-muscular contractility, at least after some time, is below par on the paralysed side, or may even be abolished altogether; and this is a point of capital importance, for in doubtful cases it will essentially aid us in diagnosing an excentric facial palsy from one of central origin. In the latter case the muscles respond normally to the electric stimulus, whether faradic or galvanic; and as this form is usually dependent on hemorrhage having taken place, we see how a wrong diagnosis might occasion a serious mistake as to treatment.

The prognosis in rheumatic facial paralysis is generally good,

and is guided mainly by reference to the suddenness with which the affection has set in, and its duration, the age of the patient, and the electric excitability of the muscles; for in proportion as this property is preserved will the case be amenable to treatment. Particular attention should be paid to the latter condition in the progress of the case, because if the muscles show little or no signs of returning excitability we may fear that the disease will prove very stubborn, if not incurable.

In tolerably recent, or even in old cases, though the reaction towards the faradic current is diminished or lost, the reaction towards galvanic stimulation may be heightened (Benedikt). Since the electrical treatment answers best in recent cases, I will briefly relate a few in the order of their duration before they were submitted to electric treatment:—

Case I.—A man (Wm. Horan), aged sixty, came to the dispensary in January, 1868, with paralysis of the left side of the face of about four days' standing. The paralysed side is not so blank as might be expected, owing to the wrinkled state of the skin consequent on his age, and the *left* eye is *more closed* than the right; this was, perhaps, due to partial spasm of the orbicular muscle. No deviation of the uvula, nor asymmetry of the arches of the palate. A weak primary current was locally directed for a few times, and he quickly recovered. A blister had been placed behind the ear previous to my seeing him, but it proved of no service. The rapid recovery of this patient at his time of life is noticeable.

Case II.—A healthy man (S. Davidson), aged twenty-five, was admitted into Sir P. Dun's Hospital in February, 1868, under Dr. A. Smith.

Ten or twelve days before admission he was exposed for some hours to a dry, cold wind, and the next morning the first symptoms of paralysis of the left side of the face appeared. For a week he sought no advice, but at the end of that time a blister was raised on the lower part of the left cheek without his deriving any benefit from it. The muscles were faradized about nine times in the course of a fortnight, and he quickly regained the use of the facial muscles. Even on the first application the orbicularis palpebrarum responded actively to the stimulus.

Case III.—A sailor (Wm. Graham), aged thirty-nine, was sent

to me by Dr. J. Little in April, 1869. A fortnight previously he fell asleep in a draught of cold air, and three or four days afterwards paralysis of the orbicularis oris was observed, and gradually others of the facial muscles were implicated. He could close the eye till a few days ago, showing that the characteristic phenomena may require some time to develop themselves. The electro-muscular contractility was but slightly diminished, and six applications of the primary current effected a cure. After the second sitting he could nearly close the left eye, and after the fourth could whistle clearly. Duration of treatment, twelve days.

Case IV.—A boy (J. Hamilton), aged eleven, was admitted into the Adelaide Hospital in November, 1867, under Dr. Richardson. A little more than a month previously he suffered from pain behind the angle of the right jaw, and one morning shortly after he awoke found the right side of the face paralysed. All the usual symptoms were present; no deviation of the tongue or uvula. Faradization with the secondary current was commenced on the 28th of November, and continued regularly through the months of December, January, February, and part of March. Contractility was diminished, and the sensibility to the current was not so great as on the healthy side. Improvement rapidly ensued at first, but more slowly after a time, and towards the end of March the accession of two feverish attacks caused a temporary discontinuance of the electricity, which was, however, resumed at the end of April. By the first week in May he could close the right eye voluntarily, and after a few more applications was discharged from hospital nearly well, having been under treatment for about six months. These four cases, which came under observation four days, ten days, a fortnight, and a month respectively, after the paralysis had set in, were all cured in about ten days, a fortnight, twelve days, and six months respectively.

An unpleasant and not infrequent sequence of Bell's paralysis is gradual contraction of the muscles that were paralysed, simulating paralysis of the *opposite* side. It is usually partial, and often accompanied by spasmodic movements, particularly when any stimulus is applied to the skin, and it is not improbable that injudicious faradization, and the use of strychnia, may have something to do with the supervention of this morbid contraction. The following is a case in point:—

Case V.—Mrs. Byrne, aged forty-one, in August, 1867,

received a blow on the right ear from her husband. Two months afterwards she found that the right side of her face was paralysed. In January, 1868, she came to the dispensary with the signs of paralysis well marked. Primary and secondary faradization were employed, and she began to improve rapidly, but I soon lost sight of her. At the end of May I saw her again, and now the mouth was distorted to the *right* side, and she feels as if there was a drag "from the lip to the eye." Twitchings in the right facial muscles are perceptible when she speaks, and are aggravated by cold; an induced current was once applied to the muscles of the *left* side.

It has been stated that such changes in the nutrition of the cornea and conjunctiva, as are observed in anesthesia of the fifth nerve, never take place in facial palsy. But such consequences, though happily not common, do now and then occur, and seem to depend on the absence of winking from the paralysis of the orbicularis, and on the exposure of the globe of the eye from the constant patency of the eyelids. For example:—

Case VI.—A shoemaker (J. Marshall), aged sixty, applied to Dr. Barton, at the dispensary, in January, 1868. Two years previously he had been in the Adelaide Hospital for seven weeks for paralysis of the right side of the face, which he ascribed to cold. He derived considerable benefit from the treatment pursued, and improved for a twelvemonth, but on the access of fresh cold the paralysis was increased. He is not much inconvenienced now in eating or drinking, but the right lower eyelid is almost completely everted, and though there is constant epiphora, there is no scalding of the cheek. There is a considerable degree of conjunctivitis which has dated from the beginning; and on the lower part of the cornea, extending partly over the pupil, there is an irregularly oval ulcer, and general haziness of the cornea, but the sight is tolerably good. No deviation of the tongue or uvula, but the right semi-arch of the soft palate is depressed and narrower than its fellow. A weak secondary current was felt acutely, and caused evident contraction in the muscles; but I lost sight of him, as he ceased to attend, and on meeting him some months after found that the eversion of the eyelid was not so marked, and that the tears no longer overflowed. No pain in the right eye; but the iris had fallen forwards, and threatened to protrude beyond the corneal ulcer.

Though, as is but natural, our treatment is most likely to

be crowned with success in cases of recent date, yet instances have been recorded of a cure, or at least notable improvement, having been effected when the disease had continued for eight, twelve, or even fourteen years, and this would warrant us in not pronouncing any case hopeless merely on the ground of its duration. As part of the treatment, it is a common practice to apply small blisters in the neighbourhood of the ear or in the track of the affected nerve; but I do not remember ever having seen any good result from such means, while the time thus lost may be most prejudicial towards recovery, and I am convinced that electricity will prove the most prompt and trustworthy means of treating this affection.

While admitting that the use of an interrupted *galvanic current* (from a constant battery), *i.e.*, galvanization, is necessary in some cases where faradization fails to elicit contractions in the paralysed muscles, in the majority of cases the most suitable form of electricity is the primary current (*i.e.*, the current from the inner or thick and shorter spiral of an induction machine) applied locally, by means of pointed conductors tipped with moist leather, by direct or indirect faradization. The secondary current (*i.e.*, the current from the outer or finer and longer spiral), causes much more pain on account of the greater intensity of the current, and the skin of the face, especially in the middle line, is acutely sensitive to electric stimulation. Sometimes only a certain number of the muscles are engaged, and even in the return to their normal state they recover with different degrees of rapidity, and hence we see the importance of directing the faradic treatment to the individual muscles. Stimulation of the muscles which have regained their proper contractility should be avoided, lest any tendency to subsequent contraction should be fostered, and, as a rule, the sittings should be short, and the use of strong currents dispensed with.

Facial paralysis illustrates well that local palsies, not dependent on central causes or on direct interruption of nervous continuity, are commonly amenable to electricity.

Paralysis from Lead Poisoning.—The cause in these cases is usually sufficiently manifest, but as the patient may not be aware of the channel by which lead has entered the system, in all cases of local paralysis of the limbs, with diminished excitability of the muscles, strict investigation should be made as to the possibility of the unsuspected presence of lead, *e.g.*, tailors may be contaminated

by holding between their teeth the measuring tape, which has been coated with an enamel of white lead; silk thread is occasionally adulterated with sulphide of lead (produced by steeping the thread in acetate of lead, and then exposing it to sulphuretted hydrogen), and persons have been poisoned by using flour that had been ground by millstones in which some hollows were filled up with lead.

The cases I shall presently instance are all examples of paralysis of the upper extremities, saturnine paralysis of the lower extremities being comparatively unfrequent.

The middle portion of the common extensor seems generally to be first engaged, the affection gradually extending to the other extensors of the fingers and wrist, and implicating the special palmar muscles, except the interossei and lumbricales, for it is important to observe that the interossei and lumbricales seldom suffer from the poison of lead. This remarkable exemption of those particular fasciculi explains why it is that if the first phalanges be supported the patient will be able easily to extend the second and third phalanges, while he is quite incompetent to do so when the hand is let hang freely. Electro-muscular inquiry has shown that the extensor communis extends the first joint *only*, and is powerless to raise the second and third, but the extension of these latter is affected by the interossei and lumbricales, which at the same time flex the first phalanx, and consequently, if the metacarpal phalanx of the fingers be fixed, these small muscles can act independently, and are capable of moving the fingers with facility.[a] If the fingers be flexed into the palm, and an attempt be made to extend the ring finger alone, it will be found impossible to extend the first phalanx of it without moving the adjacent digits, on account of the connecting processes which pass from its common extensor tendon to those of the middle and little fingers, but the extension of the two further joints of that finger can be quite easily accomplished, the other fingers remaining flexed, because the interossei and lumbricales act independently of the common extensor.

The electro-muscular contractility is usually diminished, and often entirely lost, and in severe cases there is marked atrophy of the tissues, but there is no constant proportion observed between these conditions, or between the amount of atrophy and the degree

[a] The extensors of the wrist and fingers are supplied by the musculo-spiral nerve and its branches, all the interossei and the two inner lumbricales are animated by the ulnar nerve; the two outer lumbricales derive their supply from the median nerve.

of paralysis. The electric sensibility is generally lessened, but often partially exalted.—(Benedikt.)

This diminished motor reaction is of value diagnostically, for the paralysis is not necessarily preceded by constitutional symptoms, which would render its recognition easy, and conversely "when the muscles of a paralytic limb move well under the influence of the electric current, we may fairly conclude that there is no lead in the system."—(Althaus.)

In two cases I observed that the susurrus of the excited muscles was interrupted and crepitant.

Since the larger muscles on the forearm are those chiefly impaired in function, faradization is best directed to the muscles by moist, sponge electrodes, which cover some extent of surface, but it is to be remembered that too large a sponge offers very considerable resistance to the passage of the current. When it is desirable to act on particular muscles or on small surfaces, such as the thenar and hypothenar prominences, the small conical electrodes should be employed to ensure the proper localization of the current; and in some cases, where the usual method of faradization was insufficient to evoke muscular contractions, I have resorted to electro-puncture with success. This latter method is, of course, far more painful, and requires the intensity of the current to be cautiously adjusted.

We may, I think, congratulate ourselves on the more generally recognized value of a precise electric treatment in this malady since 1857, when Sir T. Watson could only say that "for helping to restore to the damaged muscles their lost power, electricity has been thought useful, applied in the way of sparks at first and of slight shocks afterwards, along the muscular parts of the extensors of the fingers. In the electro-magnetic apparatus, he adds, we have a more accessible and probably a not less efficacious means of furnishing the requisite stimulus."—(*Lectures*, 4th edit., ii., p. 511.)

Case I.—A house painter (J. Kane), aged twenty-six, of a robust frame, and temperate, but a heavy smoker, was admitted into the Meath Hospital[a] in October, 1863. He had been at his employment for about eighteen years. Five years ago he had an attack of colic, and within the last two or three years has suffered from several similar attacks, and he is at present labouring under a severe one. About a week ago, he detected quiverings in the right

[a] For permission to publish the cases from the Meath Hospital, I have to thank Dr. Stokes and Dr. Hudson.

arm, and this was soon followed by gradually increasing loss of power, especially in the ball of the thumb, which, indeed, has never been quite strong since the first invasion of colic. Partial drop-wrist of the left hand existed, and there was advanced atrophy of the palmar and extensor muscles of the right limb. Local faradization of the muscles was commenced on the 28th October, and in *five weeks* the arms were perfectly strong, and extension of the wrists nearly restored. The contractility of the extensors of the fingers and wrists of both arms was completely abolished, and transient muscular tremors followed the first application of electricity to the right arm, while evident marks of improvement were afforded after one or two days' treatment.

Case II.—A man (P. Joyce), aged forty-four, was admitted into the Meath Hospital in December, 1864. A painter by trade; for thirty-five years he never had any saturnine symptoms, till an attack of colic three months previously. Both arms are now weakened, even as high as the shoulder muscles, and the ball of the right thumb in particular is atrophied. On the 4th of January, faradization was resorted to, and a decided improvement set in after a few days, and the muscles rapidly gained in contractile power. In three weeks his arms were very much stronger, but unfortunately I have no further notes of the case.

Case III.—In May, 1866, a robust man (William Sheffield), aged fifty-six, was taken into Sir P. Dun's Hospital. For the last forty years he has been engaged in painting, papering, or colour-grinding, and has had colic twice before his present illness, which dates back nearly two months. Both arms, and especially the right, are partially paralysed, but there is not complete drop-wrist, and no turning in of the hand. On the 12th of May I commenced faradization, and readily obtained contractions without causing pain. Very soon improvement was perceptible, and the atrophied muscles of the right thumb resumed their normal bulk and firmness, and his writing became legible. This progressive improvement continued, and in about *three weeks* he left hospital by his own wish, having nearly recovered the full use of his hands. He was cautioned against resuming work too soon, but was obliged to do so, and in October, 1867, he applied for admission into the Adelaide Hospital under Dr. Head. A fortnight after leaving Sir P. Dun's Hospital, he resumed work as usual, and improved steadily, while

so engaged, till about three weeks ago he began to suffer from pain and tenderness along the tibiæ, and his general health declined. Soon the ring and little fingers of the right hand lost their strength, and within the last week the debility and muscular wasting have rapidly increased. Tremors in the muscles of the legs have come on, and spasmodic startings are observable in the lips, eyebrows, &c., while the muscles generally are easily excited to reflex action. No colicky symptoms at present. A moderately tight ligature applied on either thigh completely checks the tremulous movements in that limb, and on tickling the skin of the leg no reflex movements occur in it, but powerful ones in the *opposite* lower extremity.

Muscular faradization was commenced on the 13th of November, and regularly continued. The atrophied muscles soon showed evidence of increased nutrition, and in *one month* he left hospital, having nearly regained the full use of his fingers.

Paralysis from Local Injury.—Traumatic paralysis, or that in which the nerves are functionally separated from the nervous centres, depends either on complete solution of the nerve-continuity or on temporary compression of the nerve interfering with its capacity of conducting nerve-force. The former class of cases, which is best clinically illustrated in the paresis sometimes following resection operations, demands a long time, probably at least six months, before any electrical treatment is admissible, and even when anatomical continuity has been re-established, the restitution of functional activity is a matter of weeks or months' patient treatment.

The latter class, to which my cases belong, not unfrequently arises from such causes as pressure on nerves from luxations, from the too tight application of bandages, or from the weight of the body resting on the nerve during sleep. Spontaneous recovery in these cases is often very tedious, and but little reliance is to be placed on the use of blisters or stimulating local applications, while the timely use of electricity will in most cases be rewarded by a speedy restoration to health.

The electro-sensibility and excitability are more or less impaired according to the degree of nerve-lesion, and by ascertaining whether electric reaction is obtained when we place one of the electrodes *above* the point of injury, we can diagnose whether the paralysis is due to the inhibitory action of pressure on the nerve or to destruction of the axis-cylinder of the nerve. In the former case, reaction

to the electric stimulus is still exhibited, and we can, therefore, at once begin our treatment. In the latter case it is abolished, and time must be allowed for the regeneration of the injured tissue.

The treatment most generally applicable is a rapidly interrupted primary or secondary current locally applied.

Case I.—J. Harvey, a boot-closer, aged thirty, a pale, anemic looking man, came to the Dispensary, Oct. 16th, 1868, with paralysis of the extensors of the right forearm, and complete drop-wrist.

A fortnight ago he fell asleep with his head resting on the outside of the forearm, the arm leaning on the back of a chair, and when he awoke the arm was paralysed.

On the 17th I applied secondary faradization; contractility unimpaired; indeed it seems higher than on the left side, and sensation normal, except the thumb, which is partially numb. He improved steadily, and by the 26th Nov. had almost completely recovered, faradization having been employed thirteen times during a space of about five weeks.

This man also presented a curious example of a local well-marked anesthetic region on the left side of the chest, from the fifth to the twelfth rib.

Case II.—D. Barry, aged thirty-five (Dispensary, November 26th, 1868), fell asleep six weeks ago, while drunk, with his right arm under his body, and awoke with the arm paralysed and the wrist dropped. Nov. 26th.—Secondary faradization to the extensors; sensation not impaired, but there was numbness at first, and the arm feels cold; steady improvement took place. On Dec. 15th, after six applications, he could work again, and by the 22nd was nearly quite well. Duration of treatment, about three weeks.

Case III.—Hannah Langford, a servant, aged twenty-four, was admitted into the Adelaide Hospital, under Dr. Barton, in the latter part of 1868. Some years ago she received a fall on the right arm, and since that it has never been quite so strong as the other arm. For the last year she has been in the service of an invalid lady, who was very stout and heavy, weighing about sixteen stone, and she has had hard work lifting her, and supporting her by placing the right hand in the lady's axilla as a crutch, the weight of the body pressing upon the angle between the thumb and forefinger.

Paralysis has been caused by the pressure of a crutch against the brachial plexus, but in this case it was the crutch itself that gave way.

The muscles of the right hand are greatly wasted, giving it a claw-like appearance; the metacarpal spaces are hollow, and the thenar and hypothenar eminences almost obliterated. She can flex and extend all the fingers, but cannot oppose the thumb except to the index finger, and she can raise a weight of about a pound resting on the wrist. No loss of sensation, but the right hand is more easily chilled than the other, and feels habitually colder. Oct. 1st.—Faradization of the muscles was commenced; the excitability of the muscles was greatly diminished. This treatment was steadily pursued from October to the middle of February, on an average of twice or three times a week. The power of resisting cold soon increased, and on the 20th Oct. she could occasionally succeed in touching the thumb to the little finger. She had been previously electrified by a medical man in the country, with injury rather than benefit, for the method adopted was to pass one *dry* electrode over the hand for at least half-an-hour, the other electrode, also *dry*, being held in her hand. She improved slowly during December and January, and on Jan. 16th was able to use a scissors; could write very well; and on the 13th February she left for her home in the country very much improved, but she was unable to remain longer in town. I have little doubt that this case could be cured in time with the regular use of faradization; but the history of the injury and the enfeeblement of the electric contractility showed that restoration of the contractility could only be effected exceedingly slowly, and that a period of many months would be necessary to obtain a perfectly successful result.[a]

Rheumatic Paralysis.—Without entering upon the debateable ground of the pathology of these affections I will limit the term rheumatic paralysis to those cases in which the palsy appeared to follow on exposure to cold or damp. Such an affection is not an unusual sequela of sciatica, and whether connected with this disease or not is prone to engage the lower extremities, and in the upper extremities the radial nerve particularly.

According to Duchenne the electro-muscular contractility is normal; but this is only true of recent cases, for when the disease

[a] I have since heard from her that the hand is in much the same condition as when she left town.

is of long standing the excitability is generally impaired, and may even be totally abolished.

Case I.—Michael Carter, a sailor, was admitted into the Meath Hospital in Aug., 1863, for sciatica, to which he had been subject for four months. In the beginning of September he felt numbness in the left leg and foot, and gradually lost the power of moving them; he can neither flex nor extend the toes or ankle, and walks with difficulty. Sept. 25th.—Commenced faradization, and the next day he felt the leg warmer and could stand better on it; excitability of muscles diminished, and he could not expel the urine in a full stream; but control over the bladder was soon re-established by passing the current between the sacrum and pubes, and across the abdomen. Oct. 31st.—He could move the ankle-joint; electro-puncture to the anterior and posterior tibial muscles. On the 19th of November he was discharged from hospital, and when I next saw him, on Dec. 8th, he had been at sea for a week, and had almost regained complete power over the foot. Duration of the treatment about seven weeks.

Case II.—Wright Prestwich, a blacksmith, aged twenty-eight, admitted into the Adelaide Hospital, October, 1867, under Dr. Walsh, having been previously in hospital at Manchester for nine weeks, and discharged as incurable. He frequently stood on the damp floor for a long time, and four months ago began to suffer from hot, darting pains in the sole of the left foot, aggravated by heat. He had syphilis four years ago.

The left leg, below the knee, is almost useless, and the foot drags heavily in walking; some numbness in the sole of the right foot also, but no loss of power.

Nov. 19th.—The strongest induced current produced no effect whatever upon the contractility of the paralysed muscles, though the sensation was even painful, but in a few days slight contractions were obtained. Electro-puncture was frequently employed, and though the needles were introduced into the nerves and deep into the substance of the muscles no contraction of the anterior tibial muscles could be provoked; no unpleasant local effects ever followed the electro-puncture. By the middle of January the flexors of the toes were nearly as strong as in the other limb, and on the 14th of February he was discharged from hospital much improved, and able to extend the toes slightly, but still without the power of flexing the ankle. Duration of treatment, three months.

In conclusion, the general principles of muscular faradization may be thus summed up:—

1. The current should be directed *locally* to the diseased part.

2. The not obtaining visible contractions at once does not warrant us in neglecting to proceed with the electric treatment.

3. *Indirect* faradization (*i.e.*, where one electrode is placed over the trunk of the nerve) is generally preferable to *direct* faradization (*i.e.*, where both poles are placed upon the muscle.)

4. The use of painful currents is to be discountenanced, and in no case should a feeling of fatigue be induced in the muscles.

5. The difference in the effect of the same electrical stimulus on different muscles is not due to any inherent variations in their electrical sensibility, but simply to physical and anatomical considerations, such as the thickness of the skin and subcutaneous tissues, and the copiousness of the motor-nerve supply of the muscles.

6. In order to lessen the resistance of the epidermis as much as possible, the skin underneath the electrodes should be kept thoroughly moist with salt water, and since *liquids* conduct better when warm than when cold, tepid water should be preferred.

7. To produce contraction of *individual* muscles the small pointed electrode should be connected with the cathode or negative pole of the apparatus, and be placed *firmly* on the skin at the "motor points," or "points of election" (*i.e.*, the points of entrance of the motor-nerves into the muscles.)[a]

ART. VII.—*Two Cases of Chromidrosis, with Remarks.*[b] By ARTHUR WYNNE FOOT, M.D., T.C.D.; Fellow of the King and Queen's College of Physicians, Ireland.

MY object in making this communication is to put on record two cases of chromidrosis, in one of which the black colouration was exhibited, while the other presented an example of the blue form of this affection. The drawings of these two cases will convey a vivid impression of the striking nature of the disease in a much shorter time than would a verbal description of the phenomena. The first case—that of the black discolouration—occurred in a young lady

[a] Diagrams of these "motor points," copied from Ziemssen, are given in Morgan's Electro-Physiology and Therapeutics. New York, 1868.

[b] Read before the Medical Society of the College of Physicians, Ireland.

of sixteen. About a fortnight before I first saw her (which was October 23, 1867) she had caught cold and got a pain in her side and back; a week after this event a sooty looking stain made its appearance, first upon the inner border of the right lower eyelid and afterwards upon the left lower eyelid, so that the discolouration had existed for a week when it came under my observation. I found a diffused blackish-looking smut of the colour of light Indian ink, or of ink and water, a greyish sootiness, upon the cutaneous surface of each lower eyelid, looking just as if the lids had been blackleaded; the lids could be partially cleaned of the discolouration by being gently rubbed with a soft handkerchief; the stuff could be quite removed by washing, but reappeared soon again; more or less of the blackness was transferred to any material pressed against the part, and there was a distinctly appreciable powder of the finest description on the parts. The sensibility of the eyelids was not much increased; they were more vascular than usual, and a tendency to perspire from the lower lids had been observed. The uterine functions were more than naturally active, the catamenia appearing more frequently and more profusely than they should. She had absolutely no appetite, was very easily fatigued, had a short occasional cough, and some pain referred to the left subscapular region, which, in the absence of any discoverable cause, was attributed to neuralgia; her complexion was sallow, and there were indications that the liver was not acting in a healthy manner. On one occasion subsequent to the appearance of the discolouration she had been "hysterical, laughing and crying." Beyond these points there was no appreciable departure from health. The treatment consisted in the administration of citrate of iron and quinine, with Fowler's solution, and removal to the country; no local application was employed at the time. The discolouration of the face caused the lady and her friends the alarm and concern which has been so often noticed in similar instances, and their inquiries of "Will it ever go away?" and "Was there ever anything like it before?" were precisely the questions which are almost always first asked in other cases of the disease. Previous to the appearance of the discolouration she had been spending some time at the sea side, and knowing that sea air, in the opinion of some, is believed to have an influence in the production of this affection, and also that change of air has frequently been the only thing which seemed to do good in such cases, I was the more willing that she should accept the invitation

of friends living in a distant and inland part of Ireland to spend some time with them, although by her leaving town I could no longer expect to have any opportunities of watching the progress of the case. The young lady herself left town with great willingness, as in the retirement of the country she expected to be relieved from much of the attention which her remarkable appearance attracted in Dublin when out of doors, notwithstanding the use of a thick veil. She was benefited for a time by living in the country, being sometimes free for several days from the discolouration. Six months afterwards I saw her again in Dublin, at which time both lower eyelids were symmetrically discoloured, also the ciliary margins of the upper lids; the blackness was easily washed off in the mornings, but reappeared in an hour or so to the same extent as before. The colour when reappearing imparted at first a bluish tinge to the skin, and when the layer of exudation was very thin it had a bluish shade; it exuded without her knowledge. On one occasion, while in the country, the blackness threatened to extend considerably, and spread towards the right side of her nose; it has been very persistent and thick since her return to Dublin. The use of arsenic was again urged, particularly as she had not given that mineral a fair trial, having an almost insuperable objection to medicines in any shape or form. My reason for urging the use of arsenic was that its action on the vaso-motor nerves of the cutaneous and uterine arteries is very marked, effecting the contraction of the arteries supplying these parts, so that hyperemia is diminished and serous and sanguinous discharges are arrested. It has been stated that a dose of arsenic taken before a Turkish bath will prevent perspiration being induced.[a] In this case there was exhausting menstruation and a secretion from the cutaneous glands of the eyelids—circumstances which certainly indicated the trial of this metal. A month after this date, when she had been subject to this affection, with intermissions, for seven months, her mother observed several nervous attacks of sobbing and crying, succeeded by such an alteration in temper and disposition that she thought her daughter was getting deranged, and as it seemed desirable that some local means should now be used to check or suppress the exudation, a lotion of equal parts of glycerine and of liquor plumbi subacetatis was directed to be kept as constantly as possible to the affected parts: the local employment

[a] C. Handfield Jones, Functional Nervous Disorders, pp. 469, 535.

of astringent solutions of lead, alum tannin, and of mercurial pomades, has appeared to be beneficial in similar cases. The exudation at this time was very abundant; it soiled or smutted her pillow cover and bedclothes, some of it came off upon the face of a lady whom she had kissed, it was not as thick upon rising in the morning as it became after breakfast time; it was at this period that the drawing of the lady's face was made. After the use of the lotion of glycerine and lead for about a week the exudation began to lessen perceptibly in quantity; much longer intervals elapsed before its reappearance after it had been washed off. In addition to the local treatment and the internal use of iron, quinine, and arsenic, she was also taking small quantities of blue pill and colocynth. From that time up to the present, upwards of eleven months, there has been no actual return of the disease; on one or two occasions, when her liver appeared distinctly to be out of order from tenderness in the hepatic region, white stools, and slight jaundice, the skin of the lower eyelids did assume a dusky tint different from that of the other parts of the face, but there was no exudation on the free surface of the skin. The lower eyelids continued to be a little more vascular and more sensitive than natural.

I feel quite convinced that the foregoing case was a genuine one of palpebral chromidrosis, and that no deception was intended or practised. The lady's father was a medical man, on whom it would have been very difficult to impose; her mother was a clever and observant woman, very much interested in and anxious about her daughter's health, and grown-up sisters were constantly about the patient; her visit to the country was in the house of a medical relative, and to none of those about her did any suspicion occur that the appearance of the face was artificially produced; so far from this they were more astonished than pleased that such a possibility should be hinted at. It is not likely that she had ever heard or read of such a disease, as the name and nature of it were quite unknown to her father. I did not perform any of the so-called crucial tests of the reality of the disease, such as cleaning the eyelids, covering them with a layer of collodion, and awaiting the reappearance of the colour beneath the film, nor did I envelop the face or head in a gauze sac, nor cover up the part with sealed bandages, nor sit for hours watching for the exit of the colouring matter; these experiments have been made on several occasions, and are useful in suspected cases,

but as I believed this a genuine case I did not consider that it was judicious or necessary to repeat them. I was much more anxious to collect a sufficient quantity of the exudation to make a chemical analysis of it, but it so happened that as soon as my arrangements for doing so were completed, and I had secured for that purpose the invaluable assistance of Dr. Emerson Reynolds, the discolouration began to abate, and the matter ceased to be secreted in sufficient quantity to admit of collection without injurious friction of the skin. Whatever disappointment I may have felt at this event was greatly mitigated by witnessing the gratification with which the lady and her friends regarded the disappearance of the exudation.

The second case is one which did not come immediately under my own observation, but I have been furnished with many of the essential particulars of it, and with this sketch in oils which faithfully represents the blue form of circumorbital chromidrosis. The subject was a married lady, about twenty-six years of age, regular in all details of menstruation, of fair complexion, and striking beauty. Without any assignable reason, both lower eyelids became bluish, simultaneously, and to an equal extent; the discolouration was unattended by pain, and increased gradually in degree until the upper and lower eyelids of each eye became of an intense blue; the colour was very persistent, and did not appear to be intermittent in its formation while the lady was under observation; it was a source of much annoyance, owing to the attention it attracted in the street and elsewhere. At the time this sketch was made the affection had existed for six months. Such are the particulars which I have received, and it is to be regretted that they are not more complete; but as the case has not been published, and as it contrasts strongly with the black form of the disease, I thought it might with advantage be alluded to on the present occasion.

As the term chromidrosis, or chromhydrose (Χρῶμα, *pigmentum*, ἱδρώς, *sudor*), is that almost universally used on the continent for this affection, I shall adopt it without at present discussing its suitability, or the other terms for which it has been substituted. The term chromidrosis is a general one, comprehending any discolouration of any part of the external skin, which is due to the exudation of a special pigment—black, brown, blue, or yellow—provided that the discolouration be on the outer surface of the skin, susceptible of removal by friction, and of being reproduced

after a variable interval of time. This definition is framed for the purpose of excluding from the category of chromidrosis all such subcutaneous discolourations as are obscured in Addison's disease, in jaundice, cyanosis, sunburn, the cutaneous pigmentations of the breasts and abdomen witnessed in pregnancy—the utero-ovarian melasma of Laycock—and the leaden hue which results from precipitation of metallic silver in the skin after protracted employment of the soluble salts of that metal; in none of these does the pigment appear on the free surface of the skin or become capable of removal by gentle friction. The three cases of blue disease described by Caleb Parry,[a] "A blue patient," "Blue child," "A blue boy," are examples of cyanemia to be referred to congenital malformation of the heart. From chromidrosis are also to be excluded that hue of the eyelids due to venous lividity observed during menstruation, and also the discolouration like that of diluted bile or a withered leaf which the skin assumes from the presence of the vegetable growth, microsporon furfur, in pityriasis versicolor.

The last memoir on the subject of chromidrosis which appeared in this country was one published by Dr. Banks,[b] eleven years ago, in which he collected the accounts of ten previously described cases, and added one from his own practice. In this essay he alludes to a case published in 1709 by Mr. Yonge, to one in 1831 by M. Billard, one by Dr. Read and Mr. Teevan in 1845, one by Dr. Neligan, and one by Dr. Law in 1855, five by Le Roy de Méricourt in 1857, and his own case in 1858, the total number amounting to eleven. Immediately after the appearance of Dr. Banks' memoir Dr. Lyons[c] published a case of cutaneous excretion of pigment, which was novel, inasmuch as it engaged a new tract of integument—that of the hands and fingers—and because the subject of it was a lady of fifty-seven years of age; and in the same year Dr. Harvey of Cork[d] reported a case, under the title originated by Dr. Neligan, of stearrhea nigricans. It does not appear that, with the exception of these two cases, any others have, up to the present, been made public in this country since the date of Dr. Banks' paper, although I am aware that some have occurred; but the attention of medical men

[a] Medical Writings, Vol. ii., p. 104.
[b] Dublin Quarterly Journal of Medical Science, Vol. xxv., p. 257.
[c] Dub. Hosp. Gaz., 1858, p. 147.
[d] Dublin Quarterly Journal of Medical Science, Vol. xxvii., p. 232.

on the continent having been strongly directed to the subject by the essay of de Méricourt, a number of cases have since been reported from France, Germany, and Flanders, so that at present thirty-six cases are on record, to which I propose to add the two brought this evening under the notice of this Society. These thirty-eight cases have been appended to these remarks, arranged in a tabulated form, whereby can be seen at a glance the age, sex, and state of the uterine functions of each patient, the colour, situation, and extent of the exudation, the name of the observer, and a reference to where a full report of the case may be found.

It may occur to some that they hear a great deal in modern times about a disease which seems new or almost unknown to them, and they may feel surprised that so many cases of it should, as it were, suddenly crop up, and there is no doubt that this feeling is a natural one and requires explanation. It is the same with chromidrosis as with Addison's disease, Bright's disease, leucocythæmia, the so-called amyloid disease, locomotor ataxy, and progressive muscular atrophy, which but a comparatively short time ago were unrecognized and unnamed until the progress of pathology and physiology brought them into existence as definite ailments; then a host of observers eagerly hastened to record examples, many of which perhaps had lain for years in almost forgotten note-books among a list of anomalous and perplexing cases. These causes have been at work with chromidrosis; it is not to be pretended or imagined that this affection is peculiar to recent times any more than is Addison's disease or any other late addition to our nosology; but until some whose names would command attention would take upon their shoulders the responsibility of bringing this subject before the medical world, individual observers did not think it worth their while to record to the incredulous a case of a strange and inexplicable disease observed now and then in persons whose age and sex alone would provoke a suspicion of its reality, whereas, as soon as chemistry, anatomy, physiology, and the microscope, in the hands of experienced persons, began to concentrate their evidence in favour of the reality of the affection, and even to offer explanations of its occurrence, and when it began to be felt that to deny its existence would be to push incredulity beyond all reasonable limits, then every case observed is recorded, and the danger is that, in the anxiety to publish, cases may obtain the name of chromidrosis which do not deserve it.

Table of 38 Cases of Chromidrosis.

No.	Date of Publication	Name of Observer	Place of Publication	Sex	Age	Married or Single	Condition of Uterine Functions	Colour of Exudation	Situation
1	1709	Yonge	Philos. Trans. abridged, vol. v. (1703-12), p. 521.	Female	16	Single	Never menstruated	Black	"Prominent parts of face."
2	1761	Lecat	Traité de la peau humaine, 1765	Female	30	Married, mother of 4 children	—	Black	Eyelids, forehead, nose, whole face
3	1775	Gallot	Jour. de Méd. et de Chirurgie, Dec. 1775	Female	26	Single	—	Black	Hands
4	1831	Billard	Arch. Gén. de Méd., tome xxvi., p. 453	Female	16	Single	Had menstruated regularly for two years	Blue	Face, neck, and upper part of chest
5	1845	Read, Teevan	Dub. Med. Press, xiv., p. 204; Med. Ch. Trans., xxviii., p. 611	Female	15	Single	Menstruation not fully established	Black	Forehead, and upper part of face
6	1849	Bousquet	Mem. de l'Acad. de Méd., 1854, tome xviii., p. 559	Female	21	Married 2 years	Menstruation quite regular	First indigo blue, then black	Cheeks, nose, forehead, chin
7	1855	Quinan, Neligan	Dub. Quart. Jour., vol. xix., p. 293	Female	21	Single	Menstruation suppressed	"Indian ink," "bluish-black"	Both under, and right upper lid.
8	1855	Law	Dub. Quart. Jour., vol. xix., p. 297	Female	—	Single	Uterine functions deranged	Dark	"Round eyes"
9	1855	Penn	Canstatts' Jahresbericht for 1855, band iii., p. 383; B. & F. Med. Ch. Rev., No. 54, p. 471	Female	33	—	Had not menstruated for 14 years	Blue	Eyelids and cheek
10, 11, 12	1857	Le Roy de Méricourt	Arch. Gén. de Méd., 5e serie, vol. ii., p. 430	Females	17-30	—	—	Black	Face and other parts of body
13	"	"	"	Female	20½	Single	Regular	Blue first, then black	Lower eyelids and part of cheeks
14	"	"	"	Female	22	Married	Sudden suppression of menstruation	Black, afterwards blue	Lower eyelids

Table of 38 Cases of Chromidrosis—continued

No.	Date of Publication	Name of Observer	Place of Publication	Sex	Age	Married or Single	Condition of Uterine Functions	Colour of Exudation	Situation
15	1858	Banks	Dub. Quart. Jour., vol. xxv., p. 257	Female	23	Single	Catamenia irregular	Blue	Round, but especially beneath the eyes, extending to side of nose
16	„	Lyons	Dub. Hosp. Gaz., 1858, p. 147	Female	57	Married	Catamenia had ceased more than a year	Blackish	Principally hands and fingers
17	„	Harvey	Dub. Quart. Jour. of Med. Sci., vol. xxvii., p. 232	Female	19	Single	Catamenia quite regular	Black	Each lower eyelid
18	„	Blaise	Gaz. des Hôp., 6 Nov., 1858	Female	26	Single	Catamenia very scanty	Blue	Forehead and eyelids
19	„	M'Intyr	Dis. of Skin, E. Wilson, 4th ed., p. 595	Female	Young	—	—	Black	Face
20	„	Wilson	Dis. of Skin, 4th ed., p. 595	Female	Young	—	—	Black	Eyelids and adjacent parts of nose
21	1859	Kirchberg	Gaz. des Hôp., 12 Mai, 1859	Female	25	Single	—	Blue	Forehead, over left eyebrow, each cheek
22	„	Maker	Arch. Gén. de Méd., tome xiii. (5e serie), p. 224	Female	19	Single	Catamenia had never been regular —	Bluish	Whole face, especially about the eyes; sternum, epigastrium, and hands
23	„	Bärensprung	Die Hautkrankeiten, Erlangen, 1859, p. 45	Female	Young	Single	—	Blackish	Forehead and cheeks
24	1860	Fauvel	L'Un. Méd., tome vime (Nov. serie), p. 404	Female	20	Married 2 years	Catamenia habitually irregular and scanty	Black	Circle round each eye
25	„	Hardy	L'Un. Méd., Mai, 1860	Female	19	Single	Catamenia quite regular and normal	Black	The eyelids of both eyes
26	1861	Gestin	Mém. sur la Chrom. De Méricourt, p. 69	Female	16½	Single	Irregular	Bluish-black	Lower eyelids

Table of 38 *Cases of Chromidrosis—continued.*

No.	Date of Publication	Name of Observer	Place of Publication	Sex	Age	Married or Single	Condition of Uterine Functions	Colour of Exudation	Situation
27	1861	Veillard	L'Un. Méd., tome 11me (Nouv. serie), p. 203	Male	47	—	—	Bluish, and bluish-green	Backs of hands, especially of the left
28	„	De Méricourt	L'Un. Méd., tome 11me (Nouv. serie), p. 85	Female	23	Married 5 years	—	Black	Eyelids, and cheeks to angles of mouth
29	„	Duval	Gaz. Hebdom., 4 Juin, 1861	Male	48	Married	—	Black	Lower eyelids
30	„	„	Gaz. Hebdom., 14 Juin, 1861	Male	46	Married 11 years	—	Black	Eyelids
31	1862	Godefroy	Mém. sur la chromhidrose, De Méricourt, p. 99	Male	32	Single	—	Brown, then blackish-blue	Lower eyelids
32	1863	Cabasse	Gaz. Méd. de l'Algérie, 25 Jan., 1863	Female	17	Single	Irregular	Blue, violet, afterwards yellow	Whole face, except forehead, nose, and upper lip
33	1864	De Méricourt	Mém. sur la chromhidrose, p. 85	Female	21	Single	Regular	Black	Both lower eyelids
34	„	De Moerloose	Annales d'oculistique, 1864, tome 52me, p. 205.	Female	23	Single	Deranged	Yellowish-brown	Whole of face, and neck to near clavicle
35	„	Warlomont	„ „ p. 97	Female	26	Single	Deranged	Bluish	Eyelids, left breast and hypochondrium, epigastrium
36	1867	Rothmund	„ „ 1867, tome 58me, p. 156	Female	17	Single	Irregular	Black	Eyelids, especially the lower
37	1869	Foot	Dub. Quart. Jour. of Med Sci., August, 1869	Female	16	Single	Disturbed	Black	Lower eyelids and part of cheeks
38	„	—	„ „	Female	26	Married	Quite regular	Blue	Upper and lower eyelids on each side

Dr. Banks' essay forms a remarkable stand-point from which, for a moment, to survey the history of chromidrosis, for in not one of the eleven cases recorded in it was any suspicion of deception entertained by any of the observers; they did not doubt the reality of the disease; this was the period of faith in chromidrosis. In the year 1861, three years after Dr. Banks wrote, we find the Société Médicale des Hôpitaux de Paris giving an open verdict on the subject, notwithstanding the strong pleading of M. Hardy in its favour, and in the same year the Imperial Academy of Medicine declined to allow de Méricourt's Essay on Chromidrosis to be published in their memoirs, on the ground that the authenticity of the cases was doubtful; although the committee which they had appointed to report on it recommended it merely for publication, without asking for the expression of any opinion on its merits, and although seven years before they had granted to Bousquet the privilege they now withheld. It will be to the everlasting credit of Irish physicians that they were among the first to recognize the reality of this disease, for as far back as 1845 we find Dr. Read, of Belfast, and in the year 1851, Dr. Law, convinced of its existence, and eloquently defending their patients against charges of simulation and deception.

The next great summary of the known cases of chromidrosis, constituting an epoch in its history, was made by de Méricourt, in 1864, by the publication of his Mémoire sur la Chromihdrose, in which he collects together the details of twenty-eight cases observed by himself and others; among these are three cases of chromidrosis in men. To this list may be added five cases which have since occurred, and five which are not included in de Méricourt's tables, bringing the total number of cases published at the present day up to thirty-eight.

The first points which will arrest attention in looking through these thirty-eight cases is that the affection is much more frequently observed in women than in men; thirty-four times the disease has occurred in women, four times in men, and that it is generally young women who are the subjects of it, the mean age of the females being about twenty-two, although the extreme ages range from fifteen to fifty-seven. In twenty-nine cases ten were married and nineteen unmarried. In by far the greater number of cases the colouration appears on some part of the face, and in half the cases the discolouration was black or blackish. There is generally some disturbance of the uterine functions and a debilitated state of health

antecedent to the appearance of the coloured exudation. All ranks of life are liable to it, the list of cases including a duchess and a washerwoman, a farm steward and a captain of a French frigate.

The fact of so many cases occurring in young women has unfortunately militated against the careful study of the disease, and certainly has greatly contributed to the disbelief of many in this affection. These young women, they say, are hysterical, they paint themselves, they put on lampblack, graphite, indigo, black lead, to acquire an interesting appearance, to attract observation. There have been one or two notable instances of deception, cases in which colouring matter was put on the face, and imposture successfully kept up for a length of time; and these cases have done more to throw doubt on the existence of chromidrosis than a host of genuine cases have to establish it. A very remarkable case of simulated chromidrosis occurred to M. Duchenne de Pavilly.[a] He was called to see a woman, thirty-six years of age, whose eyelids exhibited a blue discolouration. The extraordinary and anomalous nervous symptoms which she exhibited excited the suspicions of M. Duchenne. One day when closely examining the eyelids he found a small mushroom-shaped wart or pendulous tumour, supported by a very thin pedicle; upon removal of this he found the pedicle was white, as well as the portion of the palpebral skin protected by the shelter of the wart. He made no remark at the time, but on the next day, in presence of the mayor of the town, whom he observes was a man of great intelligence and local importance, he washed the eyelids clean, and then abruptly demanded what kind of colour she had applied to her face. After much denial and hesitation she admitted that she had been for twenty years in the habit of painting her face with indigo, that she had been put up to it by an old woman, one of her neighbours, and that her motive was indolence. She was able to make a livelihood without working by taking advantage of public credulity. The account of another very remarkable case of alleged simulated chromidrosis is contained in a letter from Dr. Spring, Professor in the University of Liège,[b] communicated to the Soc. Méd. des Hôpitaux. A young girl, of fifteen, of the upper classes, exhibited a black discolouration of the eyelids, and of other parts of the skin of the face. One evening the face was carefully cleaned, and a layer of collodion applied; the next morning the black matter was found on, and not underneath

[a] Gaz. des Hôp., 12 Mars, 1859.
[b] Un Méd. 1st Oct., '61.

the collodion. The only conclusion to be formed was that the matter had not been secreted from the skin, but applied externally. The *modus operandi* was never explained, nor was any confession elicited. The girl was ordered to travel and take salt water baths, and no more was heard of the affection. Dr. Spring admits he cannot account for the re-appearance of the discolouration under his own eyes and those of his colleagues, as they sat watching her after having cleaned the face. This young lady, prior to the collodion experiment, sat in the professor's study, accompanied by her mother, and under the observation of three physicians; the face was cleaned with the greatest care, it became coloured again at the end of a quarter of an hour, without anyone having been able to see her raise her hands to her face.

These two cases are the trump cards of the opponents of the doctrine that chromidrosis is a real disease. The worst that can be stated against the disease is stated when these cases are mentioned, and it was the feeling of uncertainty which these revelations gave rise to in the medical mind at the time that caused the Academy of Medicine and the Medicale Society of the Hospital of Paris to be so cautious in expressing an opinion on the subject in 1861. It is plain that they must be got out of the way before the evidence in favour of the disease is alluded to. As to the first case in which the deception was confessed to, no one will regard that case as any argument against the existence of the disease in other persons. There is hardly a known disease which has not been simulated. Fictitious hemoptysis is on record since the days of Galen,[a] and imitations of epilepsy, paralysis, insanity, and of many other diseases, are of daily occurrence. In the second case the value of the collodion experiment is greatly lessened by our present knowledge that this substance is endosmotic and does not prevent transpiration from the skin, and hence it has disappointed the expectations which were formed of it as an application in the treatment of burns, erysipelas, and several diseases of the skin. This young girl had been most carefully brought up, and could not have heard of chromidrosis, and certainly had never read of it, since the medical men of the town she lived in were not aware of the existence of such a disease. The

[a] "Deprehendi quidem sunt qui sub finem tussis quoties vellent, sanguinem excreverint, venulâ quâdam circa gingivas apertâ: sicque comparatâ ut quando videretur linguâ illam urgentes, statimque post tussientes, sanguinem quasi ex inferioribus expuerent."

case occurred in the year 1857. The two cases just referred to are of course not included in those which have been tabulated.

The following case recorded by Dr. Lyons[a] possesses remarkable interest:—The lady was aged fifty-seven, the mother of a healthy grown up family; menstruation had ceased about a year or two. She had gradually fallen into a bad state of health, but the phenomena which especially attracted her attention was a general discolouration of the surface, more especially observable on the hands, fingers, and thighs. She noticed her linen to become rapidly soiled, and as if lightly sprinkled with a sooty powder. Her hands, especially the tips of her fingers, were visibly blackened, not uniformly, however, but as if they had been in contact with some black powder. She complained that it was impossible to keep her hands clean; that though she washed them twenty times a day they always looked as if she had been poking her hands into the coals all day. This phenomenon was accompanied with frequent formations of pustules on various parts of the body. Under treatment the general health was amended, and the pustular formations ceased; but after two months, although there had been some diminution in the amount of pigment excreted by the skin, it had never wholly disappeared, and continued to the date of publication to be more or less prevalent.

Marriage does not appear to have any effect upon the disease, as in one of de Méricourt's cases (*op. cit.* p. 62) the black colouration continued after the birth of several children, although during lactation it notably diminished. In a second case it persisted very intensely in spite of marriage, the birth of a child, and the appearance of perfect health; it persisted equally in a third case during pregnancy. Four cases have been observed in men; one was in a farm steward, aged forty-seven; a second in the captain of a French frigate, aged forty-eight; a third in a gentleman of forty-six; a fourth in a naval lieutenant, aged thirty-two. It will be easily seen that hysteria, at least as the term is generally understood, cannot be solely responsible for chromidrosis; cases of well marked hysteria and of deranged uterine functions are common enough; cases of chromidrosis are rather rare. In a very large number of the cases the patients were married; in those married during its existence this event, so often regarded as a panacea for hysteria, had no beneficial effect, nor had pregnancy, or the bearing of children. The

[a] Dub. Hosp. Gaz. 1858, p. 147.

affection was also seen in a lady who had ceased menstruating a year or two previously, and finally, in four men of middle age employed in active and responsible duties. The two naval officers were seen by many military and naval surgeons, who were fully convinced of the reality of the phenomenon.

If hysteria be used as a conventional term for nervous diseases little understood, characterized by excitability or depression of spirits, indecision, unaccountable behaviour, not necessarily connected with any sexual peculiarity, then it may be said that hysteria is connected with chromidrosis in both men and women, for in nearly all the cases in both sexes the nervous system was in a debilitated condition, unstrung by mental or physical causes. There is neurosis from moral shock as well as from physical accident. The fact pointed out by Sir Ranald Martin that distressing hysterical symptoms with depressed nerve-power and its attendant indecision are produced in men by sunstroke, has at once contradicted and disproved the imputation of feminine weakness, or the necessary implication of an organ exclusively female. (Hovell, *on Neurosis from Moral Shock.* Lon. Hosp. Reports, 1867, Vol. iv. p., 406).

The colour which the outer surface of the skin assumes in chromidrosis is by no means uniform; in twenty-one out of thirty-eight cases it was black, blackish, or brownish; in fifteen it was blue, bluish black, bluish brown, or violet; in two it was yellowish brown or ochreous. The blue cases are actually the most numerous, as many of the black cases are examples of intense blues. Bousquet watched the excretion of the colouring matter in droplets of a dark sweat; it was first deep blue, and afterwards became black, either from accumulation of the colour or from the influence of the air; M. de Méricourt saw the colour dull blue on the day of its appearance, while on the following day it had assumed the appearance of Indian ink; Maker more than once observed the tint of the blue to vary within a very short time, particularly on one occasion when he was two hours engaged in making a drawing of his patient; the skin of the lids, when partially cleaned of the blackish matter, has frequently looked bluish; the black matter, when examined under the microscope, appears to include very many laminar particles of a beautiful violet blue. When the exudation is scanty or recent it very often imparts a bluish shade, which subsequently becomes overwhelmed in the darkness of the increasing deposit. The exudation has a great colouring power, a single drop expressed from a brush which has been dipped in glycerine, and passed over

the blackened parts, shows a greyish blue tint when spread upon a slip of glass placed over white paper. If the deposit on the surface of the skin be examined with the microscope, when it has not remained too long in contact with the air, the colour of the very great majority of the particles is a deep indigo blue, almost black, except at their borders, which being better illuminated and thinner have a certain transparency, and appear of a clearer blue.—(*de Méricourt*, p. 123.) Heat has been observed to increase the formation of the colouring matter, probably from increase of the transpiration—the exudation has ceased in winter to reappear in the heats of summer. In the case of the naval lieutenant, the chromidrosis was of a dull greyish tint, while he was cruising in temperate latitudes, and when, his vessel being ordered to Mexico, he came within the tropics, the eyelids became coloured a rich black, and perspiration falling from the forehead down the face became blackened as it rolled over the eyelids. In Billard's case, the colouration, which was of an azure blue, and stained the girl's clothes, became deep or pale according as the subcutaneous circulation of the parts was increased or diminished: when questions were put to her which raised a blush (she was sixteen years of age), she blushed blue instead of red.—(*Arch. Gén. de Méd.*, 1831 p. 455.)

The following is a remarkable case of a combination of blue and yellow chromidrosis:—Mademoiselle Joséphine Jacquet,[a] a peasant girl, aged seventeen, was employed collecting grapes in a vine-yard, when a child observed to her that she had circles round her eyes; on her way home, several persons who met her asked her why her eyelids were coloured blue. She thought at the time that the colouration was accidental, but was surprised to find on the following morning that it was still present, and had invaded the upper eyelids; it had also extended from the inner angle of the eye to the beginning of the cheek, and had assumed a deeper tint. She was menstruating at the time the colour appeared; in the intervals the colour diminished, but it increased very much at the return of each catamenial period. The first medical man who saw her thought she was suffering from cyanosis, and under his treatment, directed towards organic disease of the heart, she became worse. M. Magnin was called in; by his advice twelve cupping glasses were applied to her shoulders, for the relief of an intense headache; she

[a] Gazette Médicale de l'Algérie, 25 Janvier, 1863. (Mém. sur la Chrombidrose, de Méricourt, p. 86.)

was greatly frightened by this operation, and screamed with alarm; on the following day, the violet discolouration had extended over the whole face, with the exception of the forehead, nose, and upper lip, and continued so for several weeks. This occurred in the month of January, 1862. In the month of March, her face, especially the forehead, became covered with a muddy yellow tint, which, growing thicker and thicker, ended in taking the appearance of a layer of ochreous substance, the colouration of the eyelids meanwhile was not modified. She was brought to Paris, and shown to some of the physicians of note, more especially devoted to the study of skin diseases. Their opinions were divided; the majority only saw a deception, and declared that the pretended bluish secretion was due to a skilfully prepared dye, the rest seemed to admit the reality of the phenomenon, but expressed no opinion upon its nature or treatment. The girl soon after returned to Bourbonne, her native place, more unhappy than when she had left it; since, instead of having had any attempts made to cure her, she had been suspected. M. Magnin was not much flattered by the insinuation that he had been duped, and he set himself to ascertain the truth. On several successive evenings, after having completely removed the colour from the eyelids by washing, he covered the patient's face with a veil of white gauze, the two ends of which he brought to meet behind the neck so as to envelope the head entirely in a kind of sac; the ends were tied and sealed with care. The next morning, after each experiment, he always found the apparatus intact, and the blue layer completely reproduced. This girl was offered 500 francs a month by a man, who wished to hire her on speculation, if she would go about with him and allow him to exhibit her in public. Far from appearing to take any pride in being the subject of so strange a phenomenon, she had a great reluctance to being examined, complained that nothing has been done for her, and herself suggested the most violent remedies; she was, in short, anxious to get rid of her infirmity at any price.

A very interesting case of yellow chromidrosis is that published by de Moerloose,[a] in which a yellowish brown colouration extended over all the face, and part of the neck; the subject was a girl, aged twenty-three, a domestic servant; she had received a great shock, whence had ensued amenorrhea, and many nervous symptoms attributed to hysteria. After the lapse of a considerable time, it

[a] Annales d'oculistique, tome 52me., 1864, p. 205. Schmidt's Jahrbücher Band 126. 1865.

was observed that her face was of a yellowish brown colour, which faded away insensibly on the neck, towards the clavicular region, and which contrasted strongly with the pallor of her skin, which was white and anemic. She was by no means icteric, the pigment was removable, disclosing the white skin beneath, and was more easily removed by linen steeped in oil than in water. The observer remained after its removal, until he saw it reappear and had removed it again. The exudation of pigment was more abundant on warm days, when the temperature was high. The matter was collected and submitted to microscopic examination: among the amorphous substance of golden yellow which formed the mass of the colouring substance, were found granules of a deep black colour, presenting sometimes a bluish reflection, and on this substance alcohol, ether, and cold sulphuric acid had no action, but sulphuric acid employed warm gave a brownish tint. Gubler, in examination of the colouring substance in black forms of chromidrosis, has found particles of a clear and deep brown, which seemed to him to be of the same nature as the former (the black). Lecat records a case of chromidrosis, in which the colour was first yellow, then black; a girl of sixteen was met by a man in the dark, who insulted and greatly terrified her; in the morning the eyelids were observed to be yellow; the colour gradually extended over the face for eight days, till it was covered; the yellow tint gradually deepened into black, the flexures of the elbows and the forearms became yellow, then black; in about four months, desquamation occurred, and the skin underneath was of its natural colour.[a]

The reality of the colouration as an excretion from the skin was proved by an experiment performed upon the suggestion of M. le Baron Larrey, in 1861, upon one of de Méricourt's cases. The subject of the experiment was a young girl of eighteen; she had been suffering from amenorrhea for two and a-half years, but notwithstanding this, she looked healthy and not chlorotic. She had been subject to chromidrosis for nearly four years; the secretion had been very abundant at first, so as to necessitate its removal several times a-day with soft linen and oil. At the time of the experiment, it had been ascertained that 4, 7, 10, or 15 hours might elapse before the colour became sensibly appreciable. The four lids were coloured bluish-black, the lower more so than the upper. M. le Baron Larrey examined the lids, and remarked the powdery

[a] Laycock on Cutaneous Discolourations. British and Foreign Med. Chirurg. Review. April, 1861, p. 465.

character which the deposit possessed. His proposal to the patient and her friends, that a sealed bandage should be applied over one of the eyes, was immediately accepted without any hesitation. M. de Méricourt then completely cleaned, with linen and oil, both lids of the right eye, so that not a grain of black matter could be seen with a magnifying glass. M. Rochard, a naval surgeon, then applied an apparatus, constructed as follows:—two circular pieces of fine linen, covering the orbital region, were kept fixed over the eye by four pieces of gummed paper, each two of which crossed each other at right angles; upon each of the points of intersection of the gummed bands he placed his seal; a handkerchief folded square, under another tied round the head, completed the apparatus. This was done at 4 30 p.m. At 6 15 a.m., the next morning, M. le Baron Larrey, M. Rochard, and M. de Méricourt, repaired to the house of the patient; when the handkerchiefs were removed, it was ascertained that the apparatus had not been disturbed—the seals were unbroken—and when raised it was found that the lower eyelid was coloured bluish-black, in a quite appreciable manner. M. le Baron Larrey declared the experiment was conclusive.[a]

The following case of chromidrosis in a man is one published by M. Duval,[b] first naval surgeon-in-chief of the port of Brest:—

M. César Pradier, captain of a frigate, forty-eight years of age, of sanguineous temperament, tall, and very active, had almost constantly enjoyed good health in every quarter of the globe up to 1859. On the 16th of June of that year, while commanding the corvette "Le Meuse," stationed at Rochefort, he was seized with an attack of ague which lasted twenty hours. The next day, in the absence of the surgeon of the ship, he took a well-filled table spoonful of sulphate of quinine, and repeated the dose on the evening of the same day. On the 18th he felt considerably disturbed, ringing noises in the ears, headache, more or less deafness; in the afternoon he took another but smaller dose; the fever did not return. M. Pradier remained for several days in a state of considerable weakness, which was particularly felt in the articulations of the lower limbs. Although he was quite aware of a slowly progressing anemia he was very unwilling to speak about the symptoms he felt, and which he always attributed to the unknown but certainly considerable dose of quinine which he had taken.

[a] De Méricourt, op. cit., p. 76.
[b] Gaz. Hebdom., 4 Juin, 1861.

During the winter of 1860, in which he was much exposed to the severity of the weather, he made use of preparations of iron, but without any considerable improvement. In the month of May, 1860, he was called to Lorient, to his wife, who was attacked with a very serious and painful affection; for many nights he sat up watching her until her death put an end to his fatiguing duties. He lost his sleep; the weakness increased; tears often flowed involuntarily; his appetite disappeared; a burning sensation existed in the eyes, and soon rather sharp and painful tinglings occurred in the orbital region. The patient felt a sensation of gravel which caused an involuntary winking; about the middle of the day he perceived that the eyelids were black; the coloured semi-circle about the eyes never passed farther than three or four millimetres beyond the palpebral angles. When he rubbed the coloured zone with a white handkerchief the linen was stained with a kind of black efforescence resembling fine charcoal dust. The discolouration was not more intense in the morning than at any other part of the day, but it was observed that the secretion increased with great rapidity (on one occasion in twenty minutes) under the influence of mental impressions or during animated discussions. M. Duval upon three different occasions verified the fact that the colour reappeared after the eyelids had been carefully cleaned; he observed the dark matter sensibly accumulating at the inner angle of the eye, near the lacrymal caruncle, becoming converted into small particles. The origin of these dark spots could not for a moment be referred to deposit on the skin of coal dust, such as might at any time occur on board a steam vessel when they are embarking coals, since, in addition to the scrupulous care of his person which this officer always observed, the commencement of this strange phenomenon in the eyelids occurred three months after the termination of his command, and was at its height when M. Duval saw him at Brest, 29th September, 1860. M. Duval advised him to take a course of the sulphureous waters of Amélie-les-Bains, and to the uses of these baths he attributed his cure, which was completed towards the end of the month of December of the same year.

Another case also, similar to that of M. Pradier, is reported by M. Duval.[a]

M. de C., tall and spare, forty-six years of age, of nervous

[a] Gaz. Hebd., 14 Juin, 1861.

temperament, eleven years married, and having a family of four children, enjoyed good health up to the month of July, 1860. At this date, while making a short stay at Angers, he was seized with attacks of what was regarded as intermittent fever. Upon the fourth attack, large patches of urticaria came out, and did not finally disappear until three weeks afterwards. M. de C. came home somewhat weakened, not alone by the treatment which had been adopted, and which consisted principally of purgatives, but also by a considerable bleeding from piles, which had lasted more than a week. His health seemed to have been completely re-established, when, in the month of September, at Rennes, symptoms of gastro-intestinal disturbance appeared and took the character of what is known as mucous fever. This unhealthy condition continued up to the 20th of October, without presenting any alarming character, but notwithstanding the constant and skilful care of his physician his health did not improve. About the 1st of December the black discolouration of the eyelids appeared, and presented the same characters as in the case of M. Pradier. This deplorable phenomenon deeply affected him; he refused to see his friends, and set out for Brest in search of medical advice. M. Duval had him under observation from the 20th to the 27th of December; he prescribed quinine wine for him, and insisted upon his taking ferruginous medicines. He saw him again on the 15th of the following May. At that time there was no appreciable improvement, unless in his general health; the eyelids continued to be coloured black.

A very well-marked instance of chromidrosis in a man has been reported by M. Godefroy,[a] surgeon of the second class, attached to the corps of marine-fusiliers (expedition to Mexico).

M. de X—, naval lieutenant, thirty-two years of age, of nervo-sanguineous temperament, had enjoyed habitual good health up to 1852. At this date, while serving on board an iron-clad stationed off the coast of Syria, he was attacked with rheumatic pains in the lower limbs. At the siege of Sebastopol, while attached to a naval battery, he was seriously wounded in the right thigh. Soon after his return to France, at the close of the year 1854, he began to suffer from nervous hysteriform attacks, followed by unconsciousness, which lasted for some hours. These attacks had become more rare, when, towards the close of 1855, he observed

[a] De Méricourt op. cit., p. 99.

that the lower eyelids presented a blackish discolouration, which daily increased in extent and intensity. The colour, however, varied according to the temperature; while very well marked under the influence of heat, it became scarcely perceptible in cold weather. It was thus that during the winter of 1859 the blackish tint disappeared completely, leaving only a circle at the base of the lid such as might appear as the result of exhaustion. M. de X. thought himself relieved from the affection, but this delusion was dispelled by the return of the discolouration with the first heats of summer. In the month of October, 1861, when M. Godefroy embarked on board the same vessel with this officer, the lower eyelids presented a dull grey discolouration; but when they had arrived within the tropics, in the expedition to Mexico, the eyelids assumed a deep black colour; the discolouration was distributed in a crescentic form, the concavity above was limited by the free border of the eyelid, the convexity followed accurately the inferior margin of the orbital aperture. When the eyelids were submitted to repeated washings and energetic frictions the discolourations partly disappeared; there remained a well-marked greyish stain; the linen made use of in these operations was blackened. Heat, strong emotion, digestion, and every cause which accelerated the circulation, contributed to increase the blackish colour of the eyelids. When perspiration flowing down the face passed over the lower lids it assumed a black colour. No accumulation of colouring matter was observed between the eyelashes and towards the inner angle of the eye, as has been remarked in some of the published cases. The only trouble which this affection caused was a sensation like that of gravel, which was felt from time to time in the lower lids, especially on exposure to strong light, such as the reflexions from sandy shores. There was no alteration in vision; however, M. de X. remarked that in making astronomical observations his sight was more easily fatigued than before the appearance of the discolouration. He was most anxious to get rid of this peculiar malady. The reality of the exudation of the colouring matter was verified by MM. Erhmann, Coindet, and Claudel, surgeon-majors of the first class, as well as by M. Colson, principal physician to the marine.

The usual situation for the first appearance of chromidrosis is the lower eyelids; then in order of frequency follow the upper lids, the cheeks, forehead, sides of nose, whole face, sternal region, chest, abdomen, greater part of the front of the body, the back of the

hands. The ears, at least their concave surface, have never been observed to be affected, nor has the posterior aspect of the trunk. In by far the greater number of cases, the discolouration commences on the lower eyelids, generally at their inner borders; when the four lids are affected, the two lower are much more coloured than the two upper; the exudation is almost always symmetrical, though to this rule there are exceptions; the colour is very liable to be intermittent in its appearance, and is liable to reappear after a long absence, and when it has been apparently cured; it generally returns after the reception of some mental or bodily shock, or some deterioration of health. The formation of the colour is sometimes very rapid—it may be formed two or three, or five or six times in the twenty-four hours.

Whatever other part of the body may be affected, some part of the face is almost always first, and principally so. There is a case in which the contrary was observed in a man, a farm steward, aged forty-seven,[a] who saw a greenish-blue spot appear, from time to time on the back of the left hand, over the first inter-metacarpal space; it varied in size on different occasions, was sometimes the size of a one or two-franc piece, sometimes much larger; he at first took it for a daub of paint. The left hand was not exclusively affected; the right was also so, but not so frequently as the left. On one side, it extended as far as the middle of the back of the hand; on the other, to the metacarpal bone of the thumb, which it surrounded so as to entirely cover the thenar eminence; above, it reached the wrist, where it stained his shirt blue; it never passed further downwards than the metacarpo-phalangeal articulations. The colouring matter was removable with linen steeped in oil; it always appeared during the night and never persisted for twenty-four consecutive hours; the colour of the stain was bluish, with a strong tendency to green; it always preserved the same tint, varying only in intensity, which seemed generally directly related to the extent of the discolouration. The hands were observed to be affected, as well as other parts, in the case reported by Dr. Maker of Colmar.[b] The subject of this case was a girl of nineteen, blonde, of fair complexion and hair, who had never been regular in menstruation since the establishment of that function at thirteen. The attack reported was the third; three years before, the colouration had appeared for six or seven weeks in summer; a second attack came on in winter, and

[a] Observation de Chromhidrose, par M. Veillard.—L'Union Médicale, 1861, p. 59.
[b] Arch. Gén. de Méd., tome xiii. (5th serie), p. 224.

lasted two or three weeks; the third attack had lasted from March to September, and continued at the time of observation (26th September, 1858). Her face seemed to bear the traces of a violent contusion, or to have been daubed with a solution of indigo; the colour was most intense upon the eyelids, but it extended thence over the forehead and temples, to the roots of the hair, to the nose, and descended on the cheeks to the angles of the mouth; it completely resembled indigo, and was distinctly blue without any blackish reflexion. White linen or rough paper, when passed over the skin, became charged with the colouring matter, which was completely removed by linen steeped in oil, in a few hours the colour returned; the sternal and epigastric regions, and the hands were also bluish, but the tint was paler and less constant than on the face; all circumstances which caused an increased flow of blood to the head, increased the colouration remarkably. A microscopical examination of the colouring matter was made by M. Kœberlé of Strasbourg; from its behaviour with re-agents, he considered it very analogous if not identical with indigo blue; he considered it probable that it had been originally secreted in a liquid or soluble state, as indigo white is in vegetables, and had been dehydrogenated like the latter, and then transformed into insoluble indigo blue under an oxidizing influence. He adds, that if there has been no deception, chromidrosis will present an extremely curious phenomenon in the secretion of animal indigo from the sudoriparous glands.

There are anatomical facts which appear to offer some explanation of the preference which the exudation appears to have for particular parts of the face—the lower eyelids especially. The thinner and more delicate is the skin of a part, the more likely is that part to be affected, hence the front and not the back of the body is the usual locality for chromidrosis; the number of sudoriparous glands is also much greater on the anterior than on the posterior parts of the trunk, and these must exist wherever the colouration appears. The external skin of the eyelids is very thin[a] ($\frac{1}{5}'''$ to $\frac{1}{8}'''$); its epidermis is delicate ($0{\cdot}055'''$ to $0{\cdot}058'''$ in thickness), it possesses small sudoriparous glands in its whole extent, and almost invariably is furnished with numerous small hairs, many of which are without sebaceous glands. The small downy hairs of the lids, when examined with a lens in cases of chromidrosis, are found to retain their natural colour, fair or white, which is not the

[a] Kölliker, Man. of Hum. Microscopic Anat. p. 574.

case when colouring matters have been externally applied. In women the skin of this part, as elsewhere, is comparatively more delicate than in men. The lids, particularly the lower ones, are naturally very vascular; branches from four large arteries ramify in them, from the temporal, frontal, facial, suborbital; the palpebral veins also are relatively large, and can in delicate persons, in whom the skin of the eyelids is almost semi-transparent, be seen as blue cords transversing the subcutaneous structures. The arteries and veins of the eyelids form numerous and free anastomoses, which explains why ecchymoses are so frequent and so easily produced in this region, and why erectile tumours have such a preference for these parts. The colouration in chromidrosis usually begins to appear at the inner angles of the lids; this is the place whence the principal blood vessels are distributed to the lids, and where the vessels are largest and closest. The return of the colouration, after an intermission in its appearance, has been observed to have been preceded by swelling of the subcutaneous and submucous vessels of the lids.[a] In some cases, the capillary vessels of the lids, especially of the lower ones, have been observed to be very much developed, assuming almost a varicose appearance, and becoming very apparent to the naked eye. The eyelids, from their structure, offer very favourable opportunities for the secretion of any soluble material from the blood. Causes which increase the flow of blood to the face, such as heat, stooping, and mental emotion, have been found in many cases to intensify or to renew the colour. The sudoriparous glands of the eyelids, though small, are distributed extensively over the parts, and are capable of great activity; a tendency to perspire in this situation has been observed in palpebral chromidrosis, and was noticed in the case which I have observed. In one of the male cases, these parts were observed to perspire when no other part of the body did so. Independently of chromidrosis, sweating of the eyelids has been several times observed by Graefe[b] as a local phenomenon. A secondary conjunctivitis arose from the irritation of the sweat flowing in at the angles of the eye, which, to superficial observation, appeared the primary ailment—examination with a lens, when the lid had been wiped dry, showed that the fluid issued from many punctiform apertures in the skin, and gradually collected into drops. Bousquet observed the secretion of the colouring matter as it issued in punctiform

[a] De Méricourt, p. 124.

[b] Graefe, Archiv., Band. iv., p. 254.

drops from the wiped lid. The exemption of the ear from chromidrosis may be accounted for by the usually slight vascularity of the part, the thickness of the skin, and the scarcity of sudoriparous glands, which are wholly absent from the concave aspect of the concha.[a]

The eyelids being the prime and principal seat of chromidrosis, it is not surprising that local symptoms often, though not always, prevail in them; they are frequently very sensitive, congested, hot, puffed, itchy, and, as might be expected, the nervous derangement or neurosis, which exists in the lids, is not strictly confined to these appendages, but extends further; lacrymation and spasmodic action of the orbicular muscle being not uncommon. In the case of the captain of the French man-of-war, the appearance of the black secretion was preceded by a feeling of burning heat in the eyes, this was followed by sharp and painful tingling in the orbital region, and by a gritty sensation, which caused involuntary winking. About the middle of the day, he observed that his eyelids were black—the coloured semicircles never passed more than three or four millimetres beyond the palpebral angles; in Dr. Read's remarkable case, the young lady stated that a pricking and burning heat invariably preceded, from about a quarter to half-an-hour, the appearance of the discolouration, so that she could confidently predict when the blackness would show itself; in Dr. Yonge's case, warm flushing of the face was the herald of the approach of the blackness. Weakness of sight, was complained of in three of de Méricourt's cases, and has since been noticed. M. Paul Dauvé[b] has published the result of an ophthalmoscopic examination of the eyes of a French officer, suffering from palpebral chromidrosis, whose case, published by M. Godefroy, has been above alluded to. At the time of the examination the black colouration occupied both lower lids—a powdery dust soiled the linen which was used in wiping them, and was rapidly reproduced; the patient complained of heat in the eye, when he kept it fixed on small and close objects, or was engaged in astronomical observations, the sight then became disturbed, and lacrymation ensued. The symptoms of ocular fatigue were coincident with the appearance of the chromidrosis. The result of the examination was the discovery of hyperemia of the retina, congestive retinitis, or, rather, choroido-retinal hyperemia. Desquamation of the skin of the lids has been noticed, and was particularly observed in Dr. Read's case,

[a] Kolliker, op. cit., p. 124.

[b] Annales d' oculistique, 53me vol. 1865, p. 236.

so much so as to lead him to designate the affection as pityriasis nigra.[a] The colour in chromidrosis can generally be easily removed for a time with a sponge, and soap and warm water, by alkaline lotions, or by a camel's hair brush steeped in oil or glycerine, but the skin may be so sensitive that attempts to wash the black or blue matter off cause much pain. In Dr. Read's case, above alluded to, "the quantity of matter removed from the skin was sufficient to darken four basins of water as black as Indian ink." The drawing of that case, which was communicated by Sir B. Brodie to the Medico-Chirurgical Society of London,[b] represents, on the right side of the forehead, the discolouration of two days' duration, and, on the left side, of five, showing a very considerable difference in the quantity secreted during the longer interval.

Looking back at the history of chromidrosis, we find that thirty-eight cases of this affection are now on record since the first observation in 1709, and that thirty-four of these have occurred in women, four in men. Two facts have thrown a feeling of doubt as to the reality of this affection, over the minds of many who have not had personal experience of it; the two facts are, first, that notable deception has been practised, detected, and confessed to; and secondly, that a very large proportion of the cases have been observed in young women. Deception, or hysteria, or both combined, expresses the pathological creed of many on this subject. I do not anticipate that in considering the subject of this or any other disease, the fact that imitation of it has been practised will easily outweigh the concurrent testimony of its existence, from numerous and careful observers at different times and places, who have been satisfied by close scrutiny of the reality of the disease, and many of whom, aware that deception had been practised, acting on that knowledge, have taken such precautions as would prevent the repetition of such an occurrence. On this subject, observes Professor Laycock, it has been alleged that these cases of stearrhea nigricans are cases of simulation, and that in fact the disease is not real; but this is a conclusion as to all cases by no means warranted by the facts when a number of cases are compared. At the same time, there can be no doubt the disease may be simulated by hysterical patients afflicted with the monomaniacal cunning, not uncommon in young girls who have a desire to excite sympathy or wonder. Anything of the kind occurring in

[a] Dubl. Med. Press, 24th Sept., 1845, p. 204.
[b] Med. Chir. Trans., Vol. xxviii. p. 611.

old women or men would require great caution in this respect. He adds, that Professor Simpson has mentioned to him a case of simulation of blepharal melasma, which came under his notice in a gentlewoman nearly sixty years old. Circumstances rendered it probable that morbid vanity was the cause of the artificial blackening.[a]

With regard to the allegation that hysteria and wilful deception have very much to say to this affection, it may be observed that it has many times appeared in married women, continued during pregnancy, and after the birth of several children; in those who have never menstruated, and two years after menstruation had materially ceased; it has occurred in men; in handsome people whom it disfigured, and in plain people whom it rendered unearthly looking. De Méricourt mentions that young children screamed when they met in the streets one of his cases. So extraordinary looking did it render one girl that a speculator offered to hire her for exhibition in the country towns. Far from being pleased at the appearance of this discolouration, the young lady, whose case is given by Dr. Yonge, was frightened to distraction when her face suddenly turned black, and the fits which ensued required his attendance, and many arguments and soothing remedies before they abated their violence. For some years this disease was a terror to all the young women at Brest, because several cases had occurred there, and were very much talked about. In a case seen there by M. Hardy, the girl asked in alarm was she going to get the black disease (*maladie noire*) of which she had heard; and the same physician observes that, if persons only knew the mortification which the presence of the deformity has been to some women, they would never think of suspecting them of dissimulation.[b] The colour has appeared on parts never artificially coloured for the sake of ornament or fashion; on parts which are habitually concealed from view; under sealed bandages; before the eyes of medical men themselves sceptical on the subject; on persons shut up by themselves under lock and key, who had previously been searched for colouring materials; in a woman for some time insane, and shut out from the follies and vanities of the world in a lunatic asylum. The state of the face in chromidrosis often precludes its subjects from society; when the disease is well-marked they cannot possibly go among any but their intimate friends, unless immediately after washing off the exudation; and they must be home

[a] Brit. & For. Med. Chir. Rev. April, 1861, p. 470.

[b] Nouv. Dict. de Méd. et de Chirurgie, tome vii^me^, p. 585, 1867.

again to hide themselves before the period during which the colouring matter is excreted and developed has elapsed; it is not admired, nor does it increase their beauty; they are stared at and pointed at in the streets. Finally, it may be asked, how could it be possible that a number of men and women, many of them grown up and sensible persons, should in different parts of the world suddenly take it into their heads to disfigure themselves without any apparent object? The disease is said to be inexplicable otherwise than by colouring matter applied from without; but this is not the case, and many ingenious explanations of it have been offered.

Neligan (*Dis. of the Skin*, p. 277) believed it to be an affection of the sebaceous glands, and to belong to that form described by dermatologists as stearrhea, the specific name nigricans being applied to it; it is also thus described by Erasmus Wilson (*Diseases of the Skin*, 4th ed., p. 594). Gintrac (*Cours de Pathol. Interne*, Tom. v.), adopting the same view, has given it the name of melanostearrhea; but the secretion is not principally composed of fatty matter, the sebaceous glands of the eyelids, the most usual situation of chromidrosis, being few and rudimentary, and where these glands are numerous and large, forming a thick continuous layer under the cutis, as on the sides of the nose, the colouration is not at all frequently seen. Auzias-Turenne (*de Méricourt, op. cit.* p. 19) suggests that the palpebral form of chromidrosis may be due to a metastasis of formation of the dark pigment of the globe of the eye, comparing it to the secretion of milk from the axillæ of puerperal women referred to by the older writers. Rothmund and Baerensprung think the colour is due to particles of carbon, or to dust and dirt floating in the atmosphere, and becoming fixed in an oily secretion from the skin of the face; this theory, however, will not explain the blue form, or the intermissions to which the affection is liable. The term proposed by Dubuc[a]—blepharo-cyanosie; by Billard—cyanopathie cutanée; and that by Dr. Law—blepharo-melœna—are alike open to the objections that the discolouration is not limited to the eyelids, and is not constantly either black or blue, but almost as often one as the other. The name chromidrosis, employed by de Méricourt in 1858, implies the exudation from the skin, by transpiration, of a coloured or colour-producing substance, and has been generally accepted as the most appropriate.

[a] Essai sur la chromocrinie partielle de la peau [Thèse de Paris, 26 août, 1861].

It is worthy of mention that appearances simulating palpebral chromidrosis have been accidentally produced. Dr. Foucard de l'Espagnery mentions (*Un. Méd.*, 1864, p. 111) the case of a lady to whom he was called on account of the sudden appearance of blackish or dark-brown circles round her eyes; he found her in such a state of excitement that she could do nothing but ask him would she be always so? would her eyes ever be right again? as she ran to and from her glass. At last he succeeded in gathering from her that she had for some time been using, by the advice of an oculist, a lotion containing acetate of lead, and had lately gone to another medical man about some other complaint; the latter advised her to take a sulphur bath, on coming out of which she found her eyes encircled with the black rings; the stains were removed in six days by poultices and lotions. In similar chemical discolourations from sulphur coming in contact on the skin with salts of lead, I have found the peroxide of hydrogen immediately—that is, within a few minutes—remove the discolouration.

From the chemical and microscopical character of the colouring matter in cases of chromidrosis there is very strong reason to believe that it is closely connected, if not identical, with the indigo compounds. The researches and observations of Prout, Heller, Debuyne, Hassall, Scheerer, Schunk, and others, have shown that human urine not unfrequently deposits indigo spontaneously, and contains habitually an indigo-yielding substance known as indican, which is probably a glucoside of white or hydrogenized indigo (Odling's *Animal Chemistry*, 1866, p. 122). Herepath has shown (*Med. Times and Gaz.*, 24th Sept., 1864, p. 338) that indigo occurs in some at least of the cases of blue suppuration, and that it, or a substance that by oxidation becomes indigo, is often present in the urine of patients with albuminuria, either from Bright's disease, or as a sequela to scarlatina. He remarks that it is a notable fact that indigo very rarely appears to be eliminated directly from the body in its markedly blue colour; on the contrary, it is thrown off, as white or hydrogenized indigo, or it results from the decomposition of a body called indican, or uroxanthine, a light yellow substance, as its name imports, which, upon treatment with acids, splits up into three other substances—two pigments named uroglaucine, or indigo-blue, and urrhodine, indigo-red; whilst a saccharine substance is also separated in notable quantity, so that uroxanthine may be considered as a glucoside, capable of affording at any moment indigo

blue and indigo red by fermentation, oxidation, or the destructive action of some of the mineral acids, such as hydrochloric and sulphuric acid. To Dr. Hassall is due the first accurate report on the existence of blue urine. It was entitled—"On the Frequent Occurrence of Indigo in Human Urine, and on its Chemical Physiological, and Pathological Relations" (*Philosoph. Trans.* 1864, p. 297). Kletzinsky has shown that the urine of the horse and cow contains a comparatively large quantity of this pigment-forming substance; and the same observer has discovered that the exhibition of creasote and of oil of bitter almonds, as medicinal agents, appears to have the power of increasing the quantity of indigo obtainable from the urine of the patients who have been under treatment with these substances (*Vogel and Neubauer on the Urine*, p. 49). The urine is not blue or green at the moment of being voided, but becomes so after exposure to the air or after the application of strong acids. In Hassall's best-marked case, the urine was of a light brown colour when voided, somewhat alkaline, and had a specific gravity of 1017. In the course of three or four days it became thick and turbid, deep brown, greenish, bluish-green, yellowish-green, and finally nearly black, the scum on the surface remaining of a permanent, deep indigo-blue colour (Harley "Lects. on the Urine," *Med. Times and Gaz.*, 10th Sept., 1864, p. 272).

Those who are interested in this subject will find in Pl. VI., Fig. 6, of Dr. Thudicum's work on the Pathology of the Urine, a representation of uroglaucine in petaloid masses, after Heller; and in Pl. CXXXVII., Fig. 5 of Lebert's Pathological Anatomy, figures of the crystalline and amorphous colouring matters of blue albuminous urines; and further information in Simon's *Animal Chemistry*, Vol. ii., p. 328; Heller "On Uroxanthine, Uroglaucine, and Urrhodine," in his *Arch. f. Chem. u. Mikrosk*, 1845, p. 161; 1846, pp. 19 and 536; and 1852, p. 121. Kletzinsky, on the same subject, in the *Arch. f. Chem. u. Mikrosk*, 1853, p. 414; Scheerer "On the Formation of Indigo in the Human Organism," in the *Ann. d. Ch. u. Phar.*, 1854, Vol. xc., p. 120; and Schunk, in *Memoirs of the Literary and Philosophical Society of Manchester*, 1857, Vol. xiv., p. 239.

It is well known to every one that indigo, as it exists originally in the sap of woad, is not blue but colourless, and that in the process of its manufacture into indigo-blue it changes from white to yellow, yellow to green, and green to blue; and there is another

grade of oxidation by which blue indigo is transformed to red. Indican or colourless indigo is also found in the blood of man, and in the urine and blood of the ox (Carter, Ed. *Med. Jour.*, Aug., 1859; Watts, *Dict. of Chem.*, Vol. iii., p. 246, 1865). Indican may be detected in the urine by precipitating the urine with basic acetate of lead, collecting the precipitate which forms in the filtrate on the addition of ammonia, and decomposing it with cold dilute acids; the filtrate deposits first indigo-blue, then indirubin or indigo-brown, and afterwards other products of the decomposition of indican. The indican obtained from the urine appears perfectly similar to the indican obtained from the indigo-plant, and which is the mother-substance of indigo-pigment.

There is nothing extraordinary in the discovery of a vegetable product in the animal body; the occurrence of indigo or of an indigo-producing substance in the secretions of men and animals is an example of the inter-dependence of vegetable and animal chemistry so frequently shown in the relationship or even identity of products formed in vegetable and animal organisms. Fat, sugar, and albumen can be got from plants as well as animals; also benzoic and oxalic acids, animal quinoidine, dextrine, fibrine, casein, and other protein compounds. The Chinese make cheese from common peas by boiling them with lime; the cheese called Tao-fo is produced from the casein of the garden, not from that of the cow; and indigo has recently been added to the number of the products which are common to two of the three kingdoms of nature. In the case of blue chromidrosis observed by M. Maker, of Colmar, a microscopic examination of the colouring matter was made by Kœberlé, of Strasbourg. He considered, from its behaviour with re-agents, that it was very analogous, if not identical with indigo-blue, and considered it probable that the colouring matter had been originally secreted in a liquid or soluble state, as indigo-white is in vegetables, and had subsequently been dehydrogenated like the latter, and thus transformed into indigo-blue under oxidizing influences. He adds, that if there has been no deception chromidrosis will present an extremely curious phenomenon in the secretion of animal indigo from the sudoriparous glands (*Arch. Gén de Méd.*, 1859, tome i., p. 226). According to Kletzinsky the elementary analysis of the blue substance procurable from urine agrees completely with that of indigo-blue (*Vogel and Neubauer on Urine*, p. 46). The presence of indican or of the indigo-forming substance in the blood is thought to be very probably due to the

destruction of some of the protein compounds, and more especially to that of hematosine the red colouring matter of the blood globules, since it is a usual ingredient of that secretion, the urine, by which the products of disintegrated nitrogenous tissues are principally discharged; it is invariably observed that the blue pigment predominates in the urine in those diseases in which the greatest destruction of blood-pigment occurs, as in albuminuria, phthisis, scarlatina, and such diseases as are productive of great and excessive anemia. In the greater number of the cases of chromidrosis the appearance of the discolouration was preceded by chloro-anemia, dysmenorrhea, or amenorrhea. In three of the cases observed in men their state of health had been previously one of extreme debility. There is no offence against the laws of secretion in the appearance of indigo-blue on the surface of the skin. When indican, which is colourless and soluble, especially in an alkaline fluid, is present in the blood under certain conditions of ill health, there is nothing to prevent its being secreted by the sudoriparous glands under the very favourable opportunities for transpiration which the integument of the eyelid and other parts of the face presents—namely, a very delicate skin, and a profuse subcutaneous capillary circulation, capable of being gorged with blood from mental emotion, stooping, heat, exercise, or from paralysis of the coats of the vessels. The indican or colourless soluble indigo, on arriving at the surface of the skin with the perspiration, which latter has been noticed to affect the skin of the lids in a marked manner in many cases of chromidrosis, being very unstable, is decomposed into indigo-brown or indigo-blue; indigo-red does not appear to be a frequent result of the decomposition of indican under ordinary circumstances. The free and full exposure of the face to light, air, and heat—great colour-developing agents—favours the rapid decomposition of indican. The blackness of many of the cases of chromidrosis of the face may be due to the separation of indigo-blue in a more than usually dense condition, or from intensity of blue colour, as it is well known a thick layer of indigo paint or a cake of the colour is deep black, or from admixture of the indigo-blue with indigo-brown—a well known product of the decomposition of indican. In some cases of blue urine the liquid appears black from the intensity of the blue, but when the colouring matter is caught on a filter and washed, it then appears of a deeper tint than Prussian blue (Robin et Verdeil, *Chim. Anat., et Pathol.* Vol. iii., 491.) In chromidrosis the colour

appears first in some cases as brown, as in Mr. Teevan's case, in which the discolouration commenced as a brownish spot near the internal angle of the left lower eyelid, which, in the course of four or five days, assumed a jet black colour, and gradually extended to the entire forehead and eyelids of both eyes (*Med. Chir. Trans.*, v. 28, p. 611.) The appearance of a subtle powder, so characteristic of genuine chromidrosis, is caused by the insolubility of the products of the decomposed indican. Hardly anything is more difficult of solution than are some of the indigo compounds, which result from the decomposition of indican; Nordhausen sulphuric acid alone dissolves indigo-blue. There are similar instances of powdery deposits on the skin from previous solutions in the efflorescence of crystals of urea on the skin (Wm. D. Moore, *Dub. Med. Press*, 3rd May, 1865, p. 419); of chloride of sodium (*Lond. Med. Gaz.*, Vol. xv., p. 18, 1834) and of urate of soda. These appearances do not, of course, necessarily involve any lesion of the integrity of the capillaries, on account of the solubility of the substances previous to their excretion. Indigo taken internally gives a green colour to the urine. Dr. Dyce Duckworth took, at midnight, gr. x. of pure indigo, made into an electuary with honey. Nothing remarkable was observed in the urine passed at two and eight hours' interval; but in twelve hours afterwards it was found to be of a decided green colour. In sixteen hours the colour was the same, something like cajeput oil; in thirty hours a smoky, greenish tinge was perceptible, and in thirty-five hours there was the faintest verdant hue remaining. The dose seemed to act as a laxative on the bowels, and the stools were tinged of a greenish colour; all the pigment was carried off by the kidneys and bowels. (*Bartholomew's Hosp. Reports*, Vol. iii., p. 218, 1867.) It does not appear that the urine was examined for indican, or any means taken to develop indigo-blue from it. The urine has been tested in blue palpebral chromidrosis for blue colouring matter, but without success. (Laycock in *Brit. & For. Med. Chir. Rev.*, 1861, No. liv., p. 471.)

A remarkable connexion may be observed between the occurrence of chromidrosis and erysipelas of the face. In the two cases recorded by Dr. Neligan and Dr. Banks erysipelas of the face preceded the exudation of colouring matter; in Fauvel's it succeeded it; in Mr. Teevan's it occurred during the continuance of the chromidrosis. In almost all the other cases the appearance of the discolouration was preceded or attended with some one or more of such local symptoms as heat, conjestion, prickling and tingling

sensations, puffiness, headache, hyperesthesia. These symptoms indicate a want of tone in the cutaneous vaso-motor nerves, a vaso-motor neurosis, such as induces the asthenic sweatings observed in debilitated constitutions. Again, the state of the general health in the subjects of chromidrosis is generally below par, some constitutional function, such as that of the liver or uterus, is out of order, or the nervous system is affected by malaria, over-anxiety, exhaustion, fright, and pain; in one case the cause was the extraction of two teeth while the catamenia were flowing (de Méricourt, *op. cit.*, p. 85.) Under circumstances such as these the indigo-forming substance has been found in the urine, and it may perhaps be the coincidence of vaso-motor paralysis of the cutaneous nerves of the face affording a more favourable opportunity for the blood to rid itself of the indigo-forming substance by the skin than by the kidneys, that determines the colouring matter to the face, where it soon becomes apparent, rather than to the urine, in which it may lie hid for days unless specially looked for. In other words, persons in the chloro-anemic state, which more or less usually accompanies chromidrosis, might excrete the indigo-forming substance which is in their blood, per urinam; but if a lesion of the cutaneous nerves of the face, or neck, or hands, determines the blood in increased quantity to those parts, the secretion is more likely to take place there, and the indigo-forming substance once secreted is rapidly decomposed under the influences of light and heat into one or more of the variously coloured bodies which indican is capable of producing. The absence of sebaceous secretion from the eyelids favours the decomposition of indican at these parts, because greasy matters would prevent the access of oxygen.

There is yet a great amount of work to be done in this subject, and in that of the animal pigments in general. But the results of the investigations of the most competent persons is tending towards the simplification rather than towards the multiplication of the animal pigments, and there is reason to believe that ultimately many of them will, as Harley has hinted, prove to be different grades of oxidation of a white radical.

In the diagnosis of chromidrosis it is, as has been before mentioned, to be observed whether the discolouration in question is subcutaneous or on the outer surface of the skin; the colouring matter should also be capable of removal and be subsequently reproduced; usually a subtle powder exists on the affected parts, communicable to things rubbed against the skin. The delicate

hairs of the skin preserve their natural white or fair colour when seen through a lens in real chromidrosis; whereas, if the colour be due to things applied from without, the hairs are discoloured. Once it has been developed, this disease generally continues for a considerable time—for several months, or even years. De Méricourt speaks of two ladies at Brest in whom the disease had existed without interruption for ten years; in some cases it disappears rather suddenly, to return after an interval of some weeks or months. Menstruation, pregnancy, sudden emotion, have seemed to cause its re-appearance. The disease may become completely cured; it generally gets well of its own accord, as the phrase is, no reason for its sudden disappearance being very apparent; at other times this happy event coincides with a general improvement of health, or with change of air. There is no instance on record of any one dying of it. Local treatment, unless auxiliary to constitutional remedies, is not of much avail. Numerous lotions and washes have been employed, with no other effect than that of temporarily removing the discolouration, which is sooner or later resecreted. Oil, glycerine, or alkaline solutions most easily remove the colouring matter; it is much slower in its reappearance after the application of oil to the affected parts, perhaps because the access of oxygen to the skin is thereby embarrassed. A combination of constitutional treatment with local application of astringent spray or lotion offers the greatest promise of success; and any therapeutical indication which a careful examination of the general health may suggest should be eagerly seized on, and be followed out with perseverance for a considerable time before the cure of the affection can be reasonably expected.

ART. VIII.—*A Case Illustrating the Effect of Quinine in Typhus Fever.* By H. THOMPSON, M.D., F.R.C.S.I., &c.; Surgeon to the County Tyrone Infirmary.

ON the 23rd of April ultimo, I was sent for to see the physician of a workhouse, twenty miles from here, and found him in typhus fever, which had commenced with a day of well-marked rigors four days before.

The disease was characterized by an unusual amount of prostration coolness of surface, and feebleness of pulse, not generally found at so early a stage. His pulse was 120, and there was an eruption o

maculæ over trunk and extremities, without any petechial admixture. He had a peculiarly heavy look in his eye, and complained of headache. His intelligence was perfect, and he distinctly attributed the attack to contagion. He had taken some aperient medicine when he found himself ill, which had cleared out his bowels. He was habitually temperate and abstemious, aged thirty, not a very strong constitution, and had as yet taken no stimulants, fearing to increase his headache.

In consultation with his local medical friend, the case was recognized as likely to be a severe one, and it was decided to have his head shaved; cold cloths kept applied; a hot jar to his feet; a citrate of ammonia mixture, every fourth hour; beef-tea and milk, *ad libitum*, and two glasses of sherry, or one of brandy, largely diluted, to be used as drink from time to time.

April 25th.—Heart's impulse weaker; want of sleep and some tendency to delirium; no complaint made of the head, so it was not inquired about. Ordered:—

Half an ounce of brandy in water every second hour.
Chloroform gtts. x.
Liq. opii sed. (Battly), gtts. xv.
M. Camphoræ, ℥ ss. M.
F. haust. h. s.

26th.—Slept five hours at intervals; one sleep lasted two hours; pulse 110, very feeble; skin much warmer; wanders a little, but knows every one; bowels confined last two days. Ordered:—

Enema saponis.
Continue nourishment as before.
Brandy, four glasses daily.

28th.—Scarcely holds his ground; the maculæ have come out abundantly, and preserve a good colour. He is intelligent, though wandering when left to himself; sleeps sufficiently without medicinal aid; bowels open, and conscious of his evacuations; respiration 40; pulse 130, and very weak. His father, a medical practitioner of great experience, and his local medical friend, concurred with me in forming a very unfavourable opinion; we all looked on the case as one that would not terminate, in the most favourable event, before the close of the third week, and we felt, considering the small effect of the large quantity of stimulants and nutriment he was consuming, that it was hardly possible he could hold out for that time.

On my way home, anxiously considering the case with these

impressions, I asked myself, "Can nothing be done to cut it short? for if it goes on, he cannot hold out," and at the same moment the idea of quinine came into my head; so I wrote back by the guard of the next train suggesting the addition of three grains to each dose of citrate of ammonia mixture; this was commenced the same night at about twelve o'clock, and he got six grains. After the second dose the gentleman in attendance thought he perceived such a change, that he was afraid to continue the quinine; the patient's eye assumed a wild restless expression. He expressed a conviction that he was about to die, called his family about him, and bade them adieu, in perfectly intelligent and affecting terms. His sister, however, plied him well with brandy, which she had been previously permitted to use without limit, and when the *scene* was over he became quieter. An enema of port and beef-tea was also given, which, with his urine, he passed unconsciously, but he slept at intervals, and perspired slightly; this was early in the morning of the eleventh day from the rigor.

April 30th.—At 9·30 a.m., I found his pulse improved, fuller and stronger, and 120; respiration 48; skin warm and moist, it might even be called hot; his intelligence was not much impaired when roused, but prostration so extreme that he lay like a log scarce able to move a finger; his tongue was black and dry, and his bed a mass of filth, both urine and feces having been passed under him; in fact, they all thought he had but a few hours to live, and that there was little use in tormenting him any further. I took quite a different view, however, and having got a clean fresh bed placed beside that on which he lay, I threw off the bed-clothes and washed his whole body over with tepid vinegar and water, cleansing every part thoroughly, and drying it as it was wet.

He was then lifted into the clean bed, and though his body was as limp and flaccid as if he were dead, as soon as he was in the clean sheets he looked up, recognized me with a smile, and muttered, "I liked that kicking about." I had previously given him five grains of quinine in half a glass of brandy, pure; I ordered it to be repeated every third hour, with beef-tea and milk *ad libitum;* before I left the house at twelve o'clock he had fallen asleep, and his skin had become hot and moist.

May 1st., 4·45 p.m.—When I left him at twelve yesterday, he was prostrate and powerless as a dead body, and he was breathing 48 in the minute, with pulse 120; perspiring slightly. Since then he had got five grains of quinine every third hour in sherry, and as

much brandy and beef-tea as he could be induced to swallow. Altogether, in about thirty hours, he had taken sixty grains of quinine, and about a bottle and a half of brandy. On this evening I found him lying quietly in a light dose; breathing 24; pulse 100; skin cool, and extremities rather inclined to be cold. Instead of a prostrate condition of the muscular system, there was rather rigidity and a tendency to be troublesome, so that for some hours it had been difficult to get him to open his mouth to let in the food and medicine. When I offered it to him he seemed to wish to comply, and would slightly separate his teeth. When awake he would also kick off the bed-clothes and try to get out of bed, showing a complete restoration of muscular power, which rendered it rather difficult to restrain or control him. While I was present he woke up, stared wildly about, tossed off the bed-clothes, and then shouted out in a loud voice and with grimaces, which would have been ludicrous under other circumstances, "Oh, my belly's sore," lengthening out the last word into a prolonged shout, and repeating it several times, at the same time pressing his hands on his sides and forcing as if he wanted to empty his bowels. An enema of turpentine, oil, and assafetida was suggested, but rejected with indignation; then a draught of castor oil, and tincture of senna was offered, which he seized with avidity and swallowed at a gulp, though previously he had obstinately clenched his teeth and made a difficulty of letting anything pass them. It was, however, evident that it did not now so much matter; a portion of the poison had been eliminated sufficient to admit of the return of his nervous and muscular power, and instead of prostration there was now rather wild vigilant excitement, obstinacy of will, and considerable muscular energy, amounting to rigidity in the maxillary muscles, and altogether partaking somewhat of the character of cerebro-spinal irritation. I, therefore, brushed some blistering liquid over the nape of his neck, and ordered a draught containing half a grain of muriate of morphia, directing the quinine to be continued in two gr. doses. I left him at midnight, having had him removed into a clean bed, in which he was sleeping tranquilly.

May 2nd.—Received the following report from his sister:—He lies quietly, sleeping and taking his nourishment with appetite; eye and voice rather excited; mouth a little dry; bowels were moved by the castor oil; was inclined to ask questions, but on being told you did not wish him to talk, turned on his side and went to sleep. His father came to see him, and was gratified and surprised to see him

so strong; does not care much for beef-tea, though he takes plenty; likes his milk, and asks for it.

May 3rd, 4·45 p.m.—Sleeping tranquilly when I arrived; soon after awoke quite himself, shook me warmly by the hand, and began to make a speech, which I put a sudden stop to.

I need not go on detailing a steady progress to recovery, which went on uninterruptedly from this forward.

I give this case for what it may be considered worth, and should not have thought of publishing it had I not been asked to do so; whether the marked change which took place after the exhibition of the quinine were a case of propter or post—every one must determine for himself. The prominent points seem to lead to the former conclusion, for the following reasons:—

In a case of maculated typhus characterized by profound depression of vital power, and considered hopeless by three men of large experience, unless some change could be effected by treatment, a measure rather unusual was adopted, with the expressed intention of producing the change so much desired, and was followed by effects so marked as to call for the discontinuance of the remedy in the mind of one of the attendants.

The improvement effected was not, however, interfered with by the temporary omission of the medicine; but, on its being resumed a few hours after in larger doses, went on until the resolution of the fever, which may be said to have been complete on the second day of the exhibition of the quinine, and on the 13th of the disease, the change having commenced on the 11th, which is not one of those periods commonly regarded as critical in the natural course of the disease.

ART. IX.—*Notes on "Mean Temperature in its Relations to Disease and Mortality,"*[a] *with Special Reference to the City of Dublin.* By JOHN WILLIAM MOORE, M.B., Ch. M., Univ. Dub.; ex-Sch., Trin. Coll. Dub.

IN the seventeenth number of the Journal of the Scottish Meteorological Society (New Series) is to be found an abstract of a

[a] The subject of which this paper treats has long since attracted attention. So far back as 1797, Dr. William Heberden, jun., F.R.S., published, in the 86th volume of the Philosophical Transactions of the Royal Society, a communication on the "Influence of Cold on the Health of the Inhabitants of London." The results arrived at seventy years ago are substantially the same as those obtained from the more accurate sources of information which are at present within our reach.

valuable paper by Dr. Arthur Mitchell, of Edinburgh, which was read before that association in the month of January, 1868. The subject of the communication to which I refer is: "Are exceptions in the distribution of temperature associated with exceptions in the distribution of disease and death?"

To this momentous question the author is enabled—so far, at least, as he has carried his inquiries—to give an affirmative answer; and in his paper he proves conclusively the connexion between low winter temperatures and excessive disease and mortality, referable to the respiratory organs.

Having recognized the importance of the subject, and having become interested in its further elucidation, I was led to apply, in the case of Dublin, the conclusions arrived at from an attentive study of the reports of the Registrars-General of England and Scotland. And this with so satisfactory a result that I felt encouraged briefly to bring forward the subject in the present paper.

In my investigations I have—so far as relates to *mortality*—depended entirely on the elaborate tables published weekly, quarterly, and yearly by the Irish "General Register Office." For the meteorological observations taken at the Ordnance Survey Office, Phœnix Park, I have, however, substituted those recorded by myself in the city—for this reason, that the temperature of the registration districts of Dublin is, perhaps, better represented by observations made in the city than by those taken at a considerable distance from it.

Respecting, then, the relations which exist between mean temperature on the one hand, and disease and mortality on the other, the conclusions which have been drawn fall naturally within the limits of the two following propositions:—

I.—*In* Summer *the tendency to morbility and death has reference to the digestive organs—diarrhea and dysentery being the affections which are especially prevalent and fatal during this season. In* Winter *a similar tendency is noticed in connexion with the organs of respiration—bronchitis, pneumonia, and pleuritis, being the affections which are principally met with at this season.*[a]

II.—*In* Summer *a rise of mean temperature above the average increases the number of cases of, and the mortality from,* abdominal

[a] Cholera and phthisis are omitted in the above classification as being in their nature *constitutional* rather than *local* affections, though the prevalence of both these diseases is much influenced by seasonal variations of temperature.

disease. In Winter *a fall of mean temperature below the average swells the numbers of cases of* thoracic *disease, and increases the mortality therefrom.*

The applicability of these propositions to Dublin may best be illustrated by a series of curves, representing the mean temperature, total mortality, deaths from thoracic disease, and deaths from abdominal disease respectively.

I have in diagram I. projected curves of this description for two years, having divided each year into quarters, the first of which, embracing the months of January, February, and March, may be considered the winter season; and the third of which, including July, August, and September, may represent the summer season.

In the case of both these years (1867 and 1868) we accordingly perceive that the curve of the thoracic death-rate reaches a maximum in the first, or winter quarter; whereas that of the abdominal mortality attains its greatest height in the third, or summer quarter.

The same will appear, perhaps more strikingly, from diagrams II. and III., which represent *weekly* curves of a similar nature for periods of six months each.

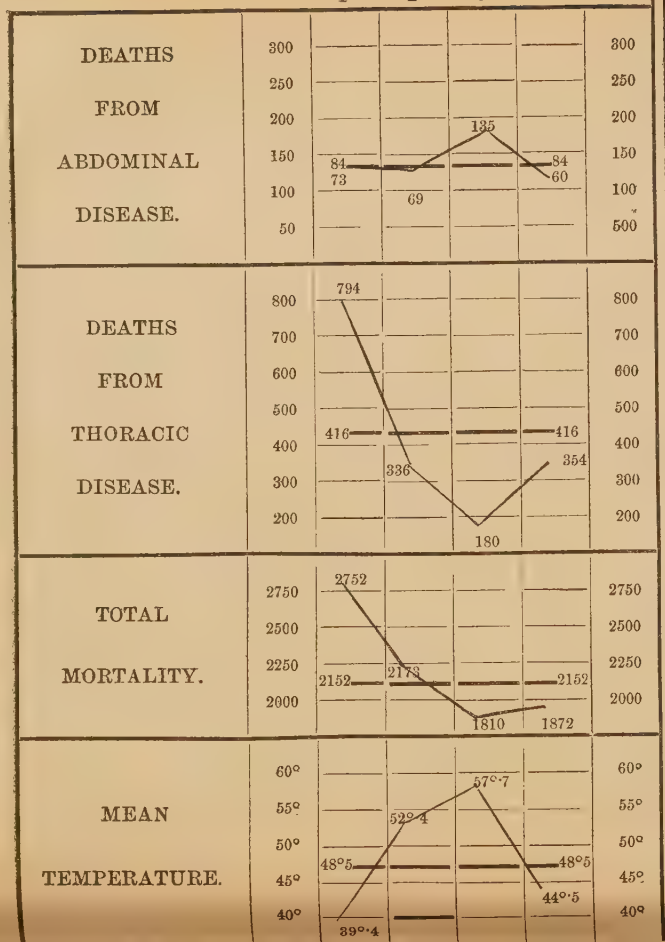
But a comparison of the corresponding curves of any two years will show that *in steepness* such curves are widely different, and this leads us to an investigation of the second proposition, which refers to the effect produced in the death-rate by *abnormal* mean temperature.

As before, we will take first the diagram which represents the quarterly returns, and afterwards those relating to periods of twenty-six weeks.

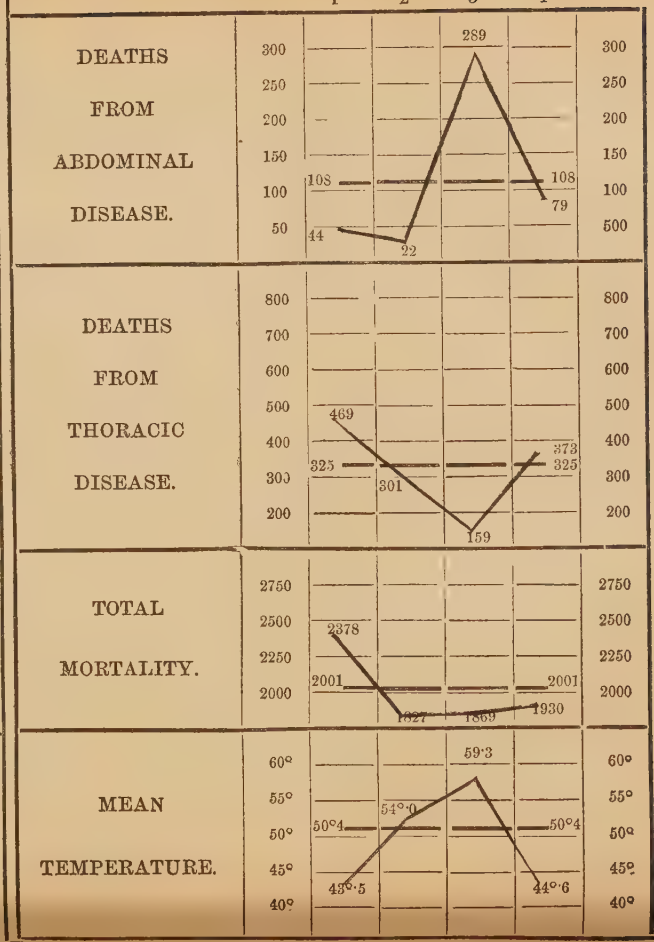
The *average* annual temperature of Dublin is 49·5°; the average temperature of the first quarter is 41·0°, that of the third is 58·7°.

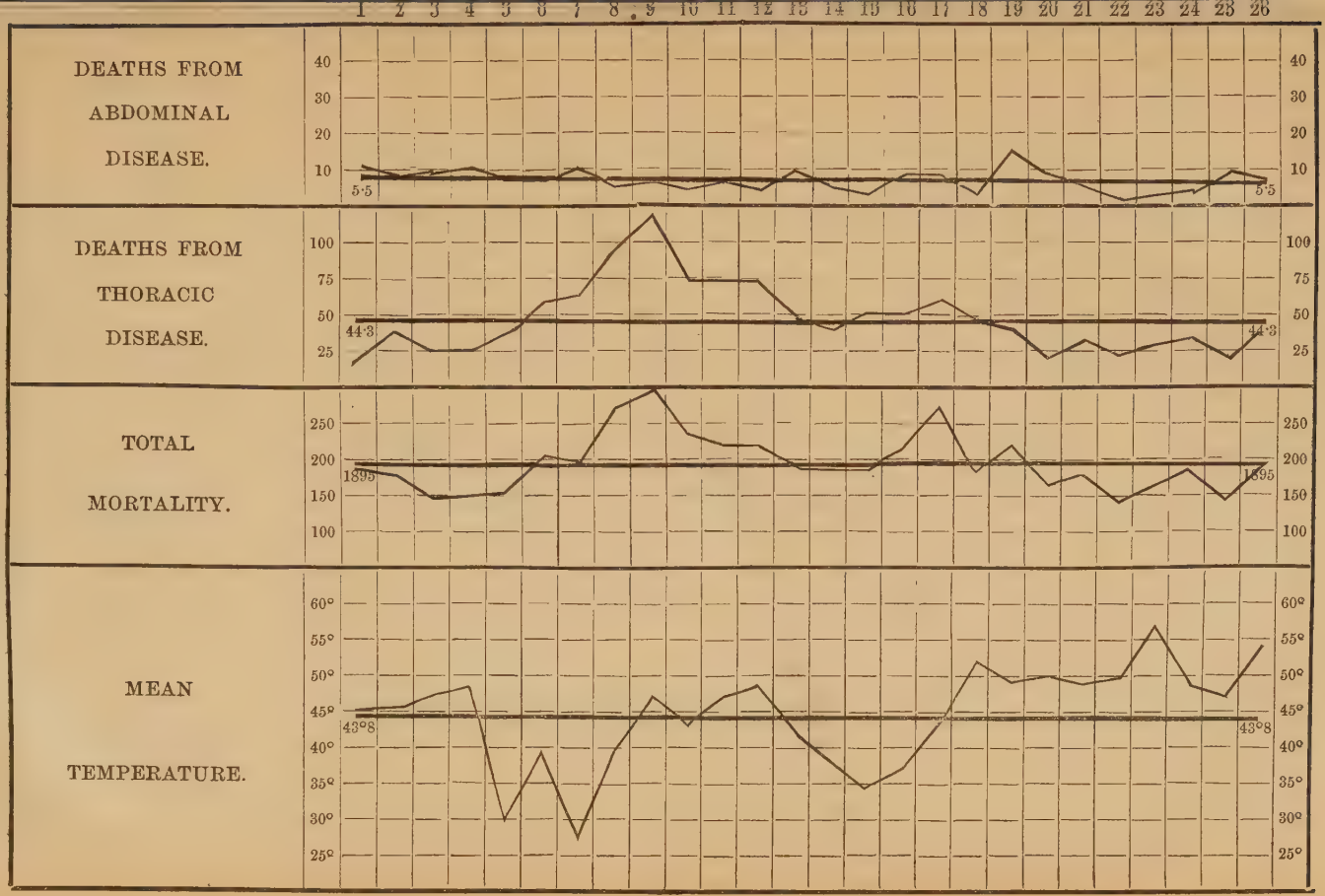
It appears that in 1867 and 1868 there existed discrepancies between the mean and average temperatures, both quarterly and yearly. Thus while the mean temperature of 1867 was 1° below the average, that of 1868 was nearly 1° above it. And, while in 1867 the mean temperatures of the first and third quarters were respectively 1·6° and 1·0° below the average; in 1868, on the other hand, the mean temperatures of the corresponding quarters were 2·5° and ·7° respectively above it.

1867. 1.—QUARTERLY RETURNS.

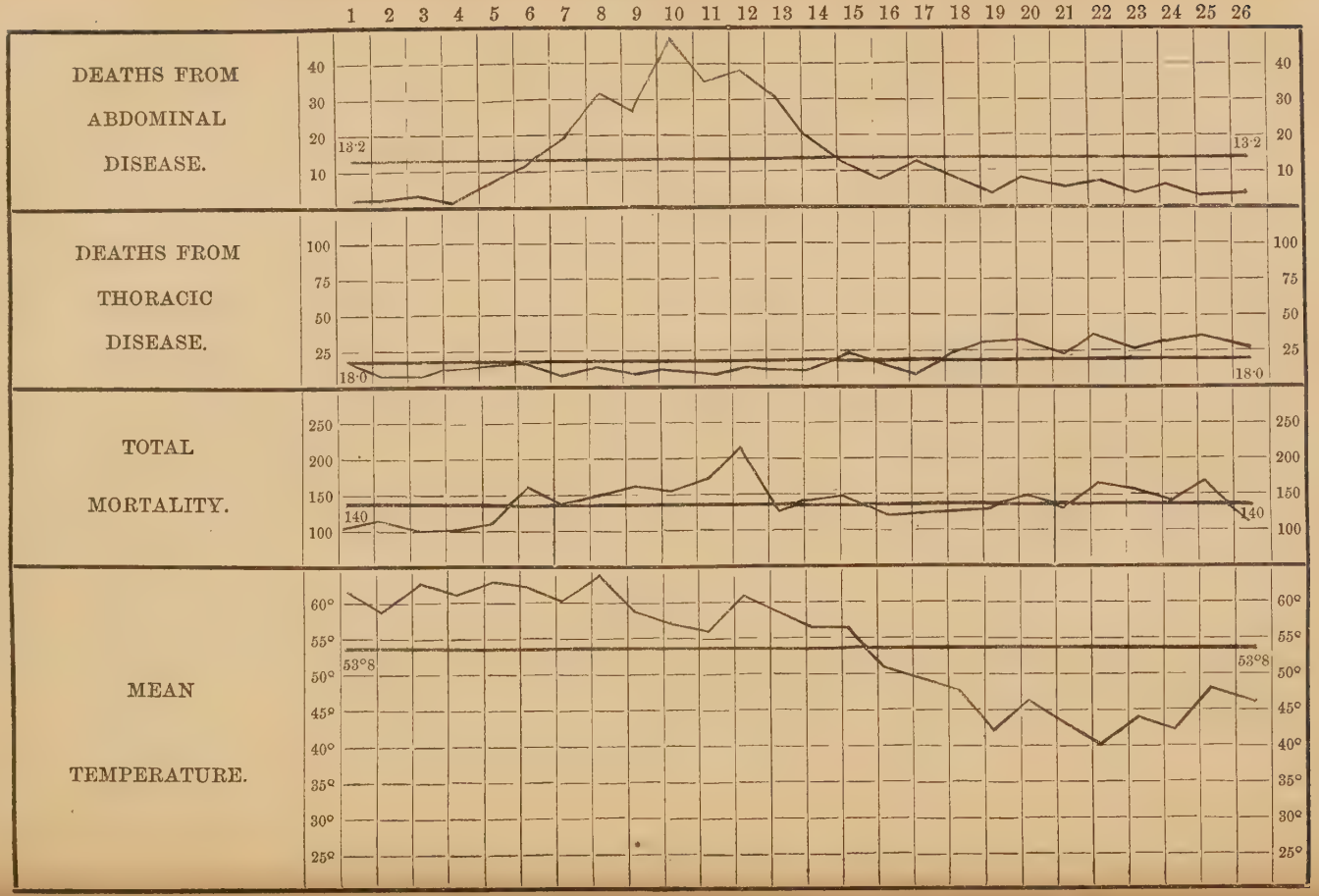


1868. QUARTERLY RETURNS.

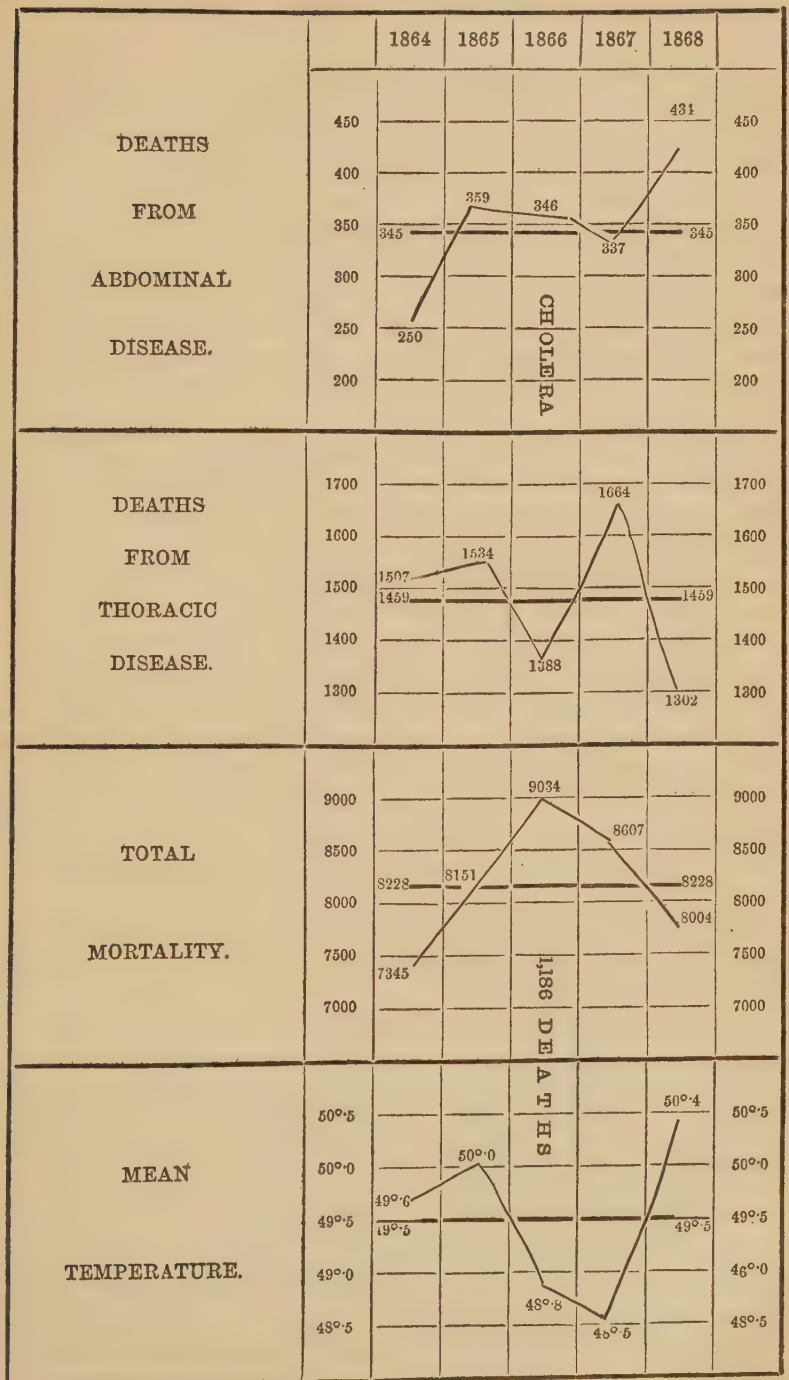



1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26
DEATHS FROM ABDOMINAL DISEASE.
40
30
20
10
5·5
DEATHS FROM THORACIC DISEASE.
100
75
50
25
44·3
TOTAL MORTALITY.
250
200
150
100
1895
MEAN TEMPERATURE.
60°
55°
50°
45°
40°
35°
30°
25°
43°8

III.—PERIOD OF TWENTY-SIX WEEKS—*June 20th* 1868, *to December 12th*, 1868, *inclusive.*



IV.—QUINQUENNIAL PERIOD, 1864–68 Inclusive.



Now, if the proposition which is under consideration stand, the thoracic mortality of 1867 should be high, more particularly so in the first quarter of that year; whereas the fatality due to abdominal affections should, in 1868, be well-marked, especially in the third or summer quarter.

And this is really the case, for it appears that in the first three months of 1867, from thoracic disease there occurred 794 deaths, or nearly half of the entire number (1,664) referred to that cause throughout the year; while in the like period of 1868 only 469 deaths happened from affections of the respiratory organs, being rather more than one-third of the whole number (1,302) occurring in the year.

So in 1867, of 337 deaths from diarrhea and dysentery registered 135 took place in the third quarter; while in 1868, of 434 deaths from these same diseases, 289, or considerably over one-half, occurred in the corresponding or summer quarter.

Turning now to the tables of weekly curves we recognize the same striking results.—(Diagrams II. & III.)

Thus, in the week ending August 22nd, 1868, a fortnight after the week of highest mean temperature of that summer which was so tropical in character, the fatality from diarrhea was excessive. The Registrar-General's note on the subject is as follows:—"The number of deaths from diarrhea *registered* during the week amounted to 49, showing an increase of 23 on the number registered during the week preceding, and *being* 35 *more* than the average deaths from this disease in the corresponding week of the four previous years."

Again, in the report for the week ending Jan. 26th, 1867, we come upon the following note in the "Weekly Return of Births and Deaths":—

"The deaths from bronchitis amounted to 80. The increased mortality from this disease is due to the extreme cold which prevailed; the mean temperature for the first three weeks of this year was 29·8°. During that period the thermometer fell to 2·8° on the 3rd instant, to 19·2° on the 12th, to 12·5° on the 16th; whereas in the corresponding three weeks of last year the mean temperature was 43·0°, and the deaths from bronchitis were only 31."

To the report of the ensuing week this note is annexed:—"The effects of the cold weather on pulmonary affections is again evidenced by the increased number (102) of deaths from bronchitis registered, being 22 more than the number registered during the week preceding."

We have so far succeeded in tracing cause and effect in the relations between mean temperature and mortality, as regards seasons and weeks. Do like relations hold in respect to years?

From a glance at diagram IV., in which are projected curves for a period of five years, from 1864 to 1868 inclusive, an affirmative answer may be given to this question, though here we experience some difficulties, in their nature more apparent than real.

Two of the five years, viz., 1864 and 1866, seem at first sight not to bear out our conclusions. Thus, while in 1864 the mean temperature was slightly above the average, yet the death-rate from abdominal causes was—contrary to expectation—below the average; that from thoracic affections having been a little above it.

Two reasons for these apparent contradictions of our theory may with justice be assigned.

First—If we analyse the elements of which the mean temperature of the year under review is made up, we shall find that though the mean temperature of the whole year was a little over the average, yet that of the first quarter was 1·5°, and that of the third quarter 0·3° below it. July indeed was warm, but August was unusually cold. In my record of the weather of this month, I find notes such as these:—"Cold northerly winds prevailed from the 8th to the 26th;" "*hail* fell on two days;" "on the 22nd the thermometer fell to 41°." September, though warm, was damp and overcast, with occasional storms from the S.W.

We may infer that before the effect of the returning heat could be felt, the falling temperature of the advanced season interposed, and prevented the occurrence of a high mortality from diarrhea.

To the great depression, 1·5°, of the mean temperature of the first three months of the same year we are to attribute the high thoracic death-rate. In addition to this, in the second place, we must bear in mind that this year was the first of registration in Ireland, and we can well understand that it took some time to set so extensive an organization as that of the registration system in working order.

With respect to 1866, two causes again combined to produce apparent anomalies in the death-rate. First, the low mean temperature of the year is to be referred rather to the coolness of the summer than to any great severity of the winter. In fact the mean temperature of the first three months was 41·3°, which is above the average; while the mean temperature of the third quarter was 56·3°, or upwards of 2° below the average. So, as was *à priori* probable, the mortality from thoracic disease was lessened.

Secondly,—To the prevalence of a terrible epidemic of cholera rather than to excessive summer heat is to be ascribed the high mortality from diarrhea and dysentery which characterized the year.

A third year, 1865, shows an anomaly in connexion with thoracic, while the high mortality from abdominal disease coincides with the high mean temperature of the summer. All discrepancy vanishes, however, when we learn that the mean temperature of the winter quarter in this year was nearly 2° below the average, January showing a depression in its mean of 3°.

In illustration of the circumstances which must, in cases of this kind, be taken into account, if we would work so as to draw reliable and trustworthy conclusions, the following extract from the *Times* of Friday, May 7th, 1869, will not be out of place:—

"The Registrar-General (of England) has to report that the state of the public health of the country, during the first or winter quarter of the present year, was not so good as in the winter of 1868. The season under review was warm, the mean temperature being nearly 3° above the average; but in addition to an epidemic of scarlet fever, trying and exceptional climatic conditions prevailed. The weather, which in the beginning of January was very warm, suddenly turned cold for about a week, and swelled the number of deaths from bronchitis and pulmonary diseases. Then a warm period set in, and lasted until the beginning of March, when there was another change to wintry and ungenial weather, which continued up to the end of the quarter, and cut off many of the very young, the weakly, and the aged."[a]

This extract shows well how careful we must be to inquire into details when drawing conclusions from seasons or years, for the Protean character of our climate, to which we may fitly apply the words "varium et mutabile semper," necessitates the attentive consideration of short periods of time.

About the years 1867, 1868, little need be said, for from the diagram the great effects of temperature on the different curves of mortality are at once evident. These effects may also be demonstrated by a statement of the respective ratios of deaths resulting from abdominal and thoracic causes to the total mortality in these

[a] In the first quarter of 1868, the mean temperature being 41·4°, 120,095 deaths were registered in England. In the same quarter of 1869, the temperature of which was nearly the same—viz., 41·3°, the deaths amounted to 133,437. The increase is due principally to the variable character of the weather in the latter year.

same two years. Thus, in 1867 there took place from abdominal affections 337 deaths, or a ratio of 1 in every 25·6 of the total number of deaths. In 1868 there resulted from abdominal causes 434 fatal cases, or a ratio of 1 in every 18·5 of the entire deaths registered. In 1867 no less than 1664 deaths from diseases of the respiratory organs took place, or a ratio of 1 in every 5·2 deaths. In 1868 1,302 fatal cases of thoracic disease were registered, or a ratio of 1 in every 6·1 of all the deaths.

It will be remembered that 1867 was the year of the *cold* winter, while 1868 was that of the *hot* summer.

Hitherto, it will be observed, I have spoken of mortality only. The influence of changes in mean temperature on morbility is no less strikingly manifested. In fact, all that has gone before applies to disease equally as to its sequel—death—the sole difference in the two cases being that the occurrence of disease is more speedily the result of changing temperatures than is the occurrence of death. Also a much more remarkable effect as regards *numbers* is apparent in the case of disease.

At this point I shall have to enlarge the borders of our inquiries, and by inference apply to Dublin such facts as have been attained by diligent investigations made elsewhere.

It is much to be deplored that, in the United Kingdom, there does not at present exist an official registration system in connexion with morbility; that, while statistics relating to mortality are elaborated with assiduous care, the even more important subject of the prevalence of disease receives little or no attention. In this we are surely far behind most of our continental neighbours.

Not many weeks since there appeared in the columns of a leading medical journal[a] an article on this subject; and we must hope that a new state of things will shortly be inaugurated, when not only will the importance of the registration of disease be generally recognized, but an organization for carrying it out will be set working.

Even now isolated statistics of morbility exist in different parts of the country, and the value attached to these should assuredly gain for the advocates of an extended system of disease registration an attentive hearing. Among such statistics those recorded from week to week in Manchester may be cited. One example

[a] Medical Times and Gazette, May 15, 1869.

drawn from this source will prove—conclusively I think—that abnormally low mean temperatures exercise a remarkable effect on disease connected with the organs of respiration. For the numbers following I am indebted to Table II. in Dr. Mitchell's paper, of which mention has already been made.

It will be remembered that I selected the period commencing December 8th, 1866, as illustrative of the effect of cold in producing a high mortality from thoracic affections. I will choose three weeks from the same period to prove that cold exercises a similar influence in raising the number of cases of thoracic disease.

In Manchester the mean temperature of the week ending December 29th, 1866, was as high as 44.0°; that of the week ending January 19th, 1867, was as low as 22·6°. From the tables above alluded to we find that the number of cases of respiratory disorders occurring in *public practice* rose from 246 in the former to 463 in the latter week; that, whereas in the first-mentioned week the ratio of thoracic to the total number of cases met with was 1 to 6, in the second week it had become 1 to 4.

Further, in the same city the mean temperature of the week ending June 15th 1867, was 58·0°, and we find that the total number of pulmonary cases which were treated in this week amounted to but 140, or in a ratio of only 1 to 9 of the total number of cases of disease.

Again, while in the five weeks ending February 2nd, 1867, 2,098 cases of pulmonary affections occurred, only 1,040 cases of the same happened in a similar period ending June 15th, 1867.

The mean temperature of the first of these periods was 33·9°; that of the second was 52·9°, or 19° higher.

The most unobservant can scarcely fail to be struck by the existence of cause and effect which is so evidently traceable in the relations between these figures.

To illustrate the intimate dependence of the tendency to disease of an abdominal type on mean temperature, I will venture to quote some facts furnished by the valuable statistics of morbility which are from year to year compiled in Sweden. In doing so, I will select more particularly the years 1865, 1866, and 1867, calling attention meanwhile to other years whenever additional proof is needed in support of our argument.

And first, it will be well to state briefly the method of arriving at these statistics, which has for some years past been in use throughout Sweden:—

It appears that in that country medical men, with very few exceptions, hold official or governmental appointments. A condition of their holding such situations is that they should send in periodically to the College of Health reports of what they have done, and statistical returns according to accurately-prepared forms. The brothers Wistrand have been the conscientious and diligent collectors and revisers of the very rich material thus obtained, which is afterwards made available to the medical public.[a]

From the tables of statistics to which I allude, and which are published from time to time in the "Hygiea," it appears conclusively that January, February, and March are the months in which the smallest number of abdominal cases is met with, while July, August, and September are those of the greatest prevalence of the same.

Thus, we find that in 1865 the number of *diarrheal* cases rose from 623 in March to 3,041 in August; that in 1866 the minimum number 663 again happened in March, while the maximum number 3,997 was attained in September; and that in 1867 the cases rose from 754 in February to 1,831 in August.

Regarding *dysentery*, the corresponding facts stand as follow:—

1865—Min.:	40	cases in	January.
Max.:	467	,,	September.
1866—Min.:	19	,,	March.
Max.:	99	,,	August.
1867—Min.:	22	,,	February.
Max.:	108	,,	July.

In the first part of a work by Dr. F. A. G. Bergman, on the Endemic Diseases of Sweden, which has just appeared,[b] there is given a tabular synopsis of all the epidemics of dysentery which have occurred in that country since the year 1452, together with a notification of the seasons of the year at which such epidemics prevailed. This Table shows that the epidemics occurred usually in July or August, lasting often until late in the Autumn; sometimes they continued sporadically during the winter, but almost always, at least on the approach of Spring, completely disappeared.

[a] For this information I am indebted to the courtesy of Dr. Edholm, editor-in-chief of the "Hygiea," who was so good as to send my father, Dr. W. D. Moore, the above account of the manner in which the data for Dr. Wistrand's valuable reports are obtained.

[b] *Om Sveriges Folksjukdomer.* Första Häftet. Upsala. W. Schultz. 1869.

On some occasions May or June has witnessed the commencement of an outbreak of the disease. When, as has rarely happened, an epidemic of dysentery has appeared in winter or early in spring, it has soon ceased without having assumed considerable proportions.

The influence of *cool* summers in checking the ravages of dysentery is also well demonstrated in the same work. Thus we find that in 1783 and 1785 violent outbursts of the disease too kplace, while it was almost quite absent in the cool summers of 1782 and 1784. The years 1808-11 and 1813 were again noted for a great prevalence of the disease, while 1812—a cold year—intervened with a remarkable freedom from dysentery. To groups of years, such as the above, the author has applied the term "dysenteric period." The facts in connexion with the two periods of this kind just alluded to, are shown in the following table:—

TABLE I.—*Deaths from Dysentery, and Temperature in Sweden.*

Years	Deaths from Dysentery	Mean Temperature			Summer
		July	August	September	
		°	°	°	°
1782	1,067	58·6	59·0	53·4	57·0
1783	8,328	64·4	60·1	54·9	59·8
1784	1,942	60·3	56·8	50·2	55·8
1785	4,436	61·2	57·4	47·8	55·5
—	—	—	—	—	—
1808	11,459	62·8	59·9	54·1	58·9
1809	11,503	63·1	63·5	53·2	59·9
1810	9,008	60·6	60·4	53·8	58·3
1811	7,204	64·6	60·3	51·4	58·8
1812	2,101	56·5	59·0	47·8	54·4
1813	6,613	63·5	58·8	54·9	59·1

So far, we have proved that summer is the season in which abdominal disease is especially prevalent. But the summer of one year differs much from that of another in point of morbility of

abdominal type. It is here that the question of mean as contrasted with average temperature engages our attention. From Table I. we have arrived at satisfactory conclusions on this point in relation to dysentery. As regards diarrhea, if we compare the years 1865, 1866, 1867—already alluded to—with 1859, we find the following results, which, for the sake of convenience, I have tabulated thus:—

Table II.—*Diarrhea and Temperature in Sweden*, 1859, 1865, 1866, 1867.

Years	Mean Temperature		Cases		Deaths		Ratio of Deaths to Cases.	
	Summer	Year	Summer	Year	Summer	Year	Summer	Year
	°	°						
1859	53·6	42·8	Not specified	7,000	Not specified	48	Not specified	1 to 146
1865	60·4	42·7	7,320	15,495	112	201	1 to 65	1 to 77
1866	58·6	43·1	10,011	19,531	69	170	1 to 145	1 to 115
1867	57·1	38·0	4,913	13,075	48	146	1 to 102	1 to 89
Average of the 4 years	57·4	41·6	7,415	13,775	76	141	1 to 104	1 to 107

1866 is an exceptional year, as the number of diarrheal cases was largely augmented by the cholera epidemic which raged from the end of June to the beginning of December. Leaving this year, accordingly, out of account, we see that with an increase of summer mean temperature equivalent to nearly 7°, a corresponding increase of diarrheal cases from 7,000 to 15,495 took place; that with a fall of summer mean temperature of rather over 3°, a corresponding fall of diarrheal cases from 15,495 to 13,075 occurred; and that the year 1867, which in the mean temperature of its summer approaches most nearly to the average temperature of the four summers under consideration, corresponds also most closely with the average number of cases of diarrhea.

If we may apply in the instance of Dublin the highest relative mortality of diarrhea to the number of cases occurring as deduced from the foregoing table, it will appear that in the week ending August 8th, 1868, so many as 3,100 cases of diarrhea occurred within the registration districts of that city.

We have now established, in the case of Dublin, the two propositions with which we set out—the first having reference to the

relative death-tendency of seasons; the second to the influence of abnormal mean temperatures on the curves of disease and mortality of an abdominal and thoracic origin. It remains for us merely to inquire what are the classes of the community on which climatic variations exercise their baneful influence in the highest degree.

The answer to this question has been already given incidentally in the words of the Registrar-General for England, "the *very young*, the *weakly*, and the *aged*." The appended facts, culled from the Irish Registrar-General's returns, will prove this statement:—

In 1867 32·5 per cent. of all the deaths registered in Dublin were those of children under 5 years of age, and 19·5 per cent. were those of persons aged 60 and upwards.

The corresponding percentages for 1868 were 23·1 and 18·9 respectively.

In the first quarter of the former of these two years—so noted for its intense cold—the percentages were:—of those under 5, 24·9; of those over 60, 23·5.

In the third quarter of the latter year—that of great heat—the numbers were:—of children under 5, 41·5; of adults above 60, 16·9 per cent.

In conclusion, while claiming for the subject of "Medical Meteorology," in its hygienic relations, the attention which it deserves, I should be misunderstood if it were supposed that by it I hoped to explain everything connected with epidemics of various diseases. Of the causes and nature of these visitations we are still profoundly ignorant. Though this is the case, from a correct appreciation of the relations between temperature and disease much good has already resulted, much more will yet accrue.

We, it is true, have no control over changes of temperature, and the other weather phenomena which may be viewed in the light of *predisposing* causes of disease; but this is not so in respect of many *exciting* causes of the same.

These are, for the most part, tangible and remediable; and on us, as medical men, devolves the responsibility of averting and of remedying them so far as lies in our power.

ART. X.—*Case of Scleriasis with Observations.* By JOHN K. BARTON, M.D., F.R.C.S.I.; Surgeon to the Adelaide Hospital; Lecturer in Surgery in the Ledwich School of Medicine, &c.

THE disease, which was originally described by Thirial under the name of "*Sclereme des adults*," has by modern German authorities been called *skleroma* or *sklerodermia*. Virchow recommends *skleriasis*, and this name has been adopted by Dr. Fagge, in an elaborate article upon this and some allied affections in the 13th Vol. of *Guy's Hospital Reports*, and may, I think, be advantageously retained, expressing as it does the prominent and characteristic feature of the disease, viz.:—a peculiar *hardness* of the integument, and involving no theory of its nature or cause. This disease is of sufficient rarity to make it desirable to record any well-marked example of it, and its true pathology, progress, and proper treatment, are as yet so obscure as to render it a subject of much interest.

CASE.—Mary Jane Campbell, a girl of twenty-three years of age, an assistant in a printing establishment, applied to me for admission to hospital, in April, 1869, on account of a hoarseness and feeling of pain and constriction in her throat, which had then lasted about three weeks, and seemed to be increasing in intensity. Upon examining the throat externally, the neck was found to be the seat of a peculiar kind of diffused induration, which extended across the larynx in front, and laterally as far back as about the edge of the trapezius muscle, the back of the neck feeling natural; the larynx seemed to be the centre of this induration, and felt larger than natural, while at the same time its prominent cartilages could scarcely be distinguished. When pressed the patient complained of pain, but the swelling at all other parts of the neck was painless; the affected skin was pale, and of natural temperature. Upon further examination the induration was found to extend down over the skin of the chest as far as the mamma, the upper part of which was hard, but the skin over the lower part of these glands was supple and healthy. Upon turning to the face to see if the disease had involved it at all, it became apparent that the skin over the entire face had a smooth, tightened, peculiarly drawn look, which made it almost impossible to pinch up the skin, and gave a remarkable expression to the countenance. When told to frown the patient's utmost efforts could only produce a slight wrinkling of the skin of the forehead, while smiling produced creases around her mouth, such as might be the result of folding parchment.

The only history the patient could give was, that about four months previously she had began to find her strength and appetite failing; she was at times sick after meals, sometimes vomiting; she suffered at the same time pains in the shoulders and back, which were worse at night. Three weeks before applying for advice she had felt some difficulty in swallowing, and at the same time began to loose her voice. This increased slowly until she was not able to raise her voice above a whisper; she became at the same time troubled with a constant cough; she was admitted to the hospital upon the 29th of April. Within a week after her admission the peculiar hard swollen state of the integument had extended round from the anterior and lateral region of the neck to the back of the neck and shoulders—where its characters were very marked—the affected integument felt brawney, like the skin of swine; it could not be pinched up. At this time the colour was perfectly natural, but it subsequently became of a yellowish tinge; no pressure would make it pit. The treatment adopted upon the patient's admission to hospital consisted of small doses of Grey and Dover's powder internally, and the *Lin. Hyd. Camph.* externally, rubbed over the whole of the affected skin once a day. Within a week the voice had become nearly natural, and the patient declared herself much better. A note taken upon the 8th of May, states that "an improvement has taken place in all the symptoms; the hoarseness has nearly altogether disappeared, and the peculiar drawn expression of the countenance does not exist to the same degree; the patient no longer complains of tightness of the throat." The skin over front and sides of the neck which had been chiefly affected, gradually became softer and more natural, and could be pinched up, but at the same time the back of the neck which, when the patient was admitted, was free, remained markedly affected, and the whole skin over both shoulders and scapulæ, and over the deltoid region on both sides, the induration gradually decreased down the arm, until at the elbow the integument felt natural, but upon the back of the right forearm, a patch of skin about the size of a crown piece was indurated in the same way as the rest of the diseased integument. When the arm on either side is moved up and down the movements of the scapulæ are not seen as usual beneath the skin; they are, as it were, concealed by the thick integument, and when the hand is laid on the part and the scapulæ moved this became very evident to the touch. No improvement took place in the state of the skin over the back and shoulders before the patient

left hospital, but the face became less drawn in its expression, and at the same time got a furfuraceous look, the cuticle coming off in small heavy scales. She left hospital upon the 29th of May, and returned to her employment of book-folding. There was not at any time any inflammatory or feverish symptoms whatever. The sensibility of the affected skin was tested upon different occasions; it was diminished, but not very much so in any part. The lower part of the body remained quite free from the disease. The urine was free from albumen, and the menstrual functions were healthy.

The great majority of recorded cases of this disease have been observed by French and German authors; however, Dr. Robert M'Donnell, of this city, has related two similar cases in the *Dublin Hospital Gazette*, Vols. for 1855 and 1856. The patients were in the great majority of cases young women. Of seventeen, collected by Dr. Fagge, fifteen were women, and two men, their ages varying from four to fifty-six, but the majority being between twenty and thirty.

The *cause* of the disease was in most of these cases, as in the one now related, obscure; but in some of the best marked instances it was plainly produced by arrested perspiration. Thus Henke relates the case of a girl aged twenty-four, who, one hot summer's day when heated with field work, lay down and slept upon some damp grass in a cool cellar, on her back, her shoulders being bare. When she awoke she felt her neck stiff, so that its movements were difficult and painful; in a few days an induration of the skin of the whole upper half of the body was established. And in another case recorded by Bouchut, and quoted by Dr. Fagge, a man, twenty-three years of age, was exposed to a draught of cold air for several hours while covered with sweat, and upon the next day the symptoms of scleriasis were established. The disease begins most frequently in the *neck*, and thence extends, as in my case, to the upper half of the body, but seldom affects the lower limbs; sometimes a few patches of induration will be found isolated, as it were, from the rest of the affected integument. The symptoms which characterize this disease are sufficiently striking to prevent its being mistaken for ordinary œdema; a peculiar thickening of the whole tissue of the skin, which renders it tense and brawney to the touch, and makes it impossible to pinch it up, without any redness or discolouration at first, but subsequently a faded yellow colour may be noticed over the affected part; sensation is dulled over the part, but not lost. The patient's own sensations are simply a feeling of

constriction or tightness, accompanied by general aching pains in the shoulders or arms, but no acute pain in the part affected, which may be handled freely without complaint.

The natural course of this disease appears to be, slowly to extend for some months from the point where it begins (usually the neck), and slowly to subside and disappear. The subsidence begins, as we might expect, in the part first affected, and the improvement extends very gradually to the parts subsequently affected. In several of the continental cases, the patients have been kept under observation for years, and a gradual improvement, and finally a disappearance of the disease took place without any treatment beyond that adopted at first, and which was not very active. The rapidity with which the symptoms have appeared and progressed varies considerably; in most cases two or three weeks elapsed before the disease was decidedly established, and then as many months before any subsidence took place; in some, years elapsed without any decided change; in one of Dr. M'Donnell's cases the disease had existed a year, and the patient remained under treatment for more than a month without any improvement whatever. In one case only has an opportunity of examining the pathological anatomy of this affection been afforded. Forster, who made the examination, thus sums up his observations —"The scleroma of this case was characterized by a chronic process of proliferation in the connective tissue of the corion, and particularly of the subcutaneous tissue, unattended with fever or local inflammatory symptoms;"[a] the fat usually present beneath the skin had completely vanished, being replaced by the hypertrophied connective tissue. The treatment hitherto adopted, although including a great variety of local applications and internal remedies, has not seemed to make any change whatever in the disease. In my case the laryngeal complication induced me to give for several days alterative doses of Grey and Dover's powder, which was quickly followed by a subsidence of all laryngeal distress and hoarseness, subsequently Lin. Hyd. Camph. was rubbed daily over the affected skin for about a fortnight; no decided effort appeared to be made by this; but the patient, as already stated, left hospital, relieved to a great degree of pain and the feeling of constriction, and able to return to her employment, but still exhibiting the peculiar characteristic induration over the shoulders, neck, and face; warm baths, and cod-liver oil rubbed over the affected integument has appeared in some cases to be of use.

[a] Guy's Hosp. Rep., Vol. xiii., p. 313.

ART. XI.—*Ascending and Descending Respiration: an Inquiry into its Cause and its Diagnostic Value.* By J. HAWTREY BENSON, M.B., T.C.D.; L.K. & Q.C.P.I.; Physician to the City of Dublin Hospital.

THE phenomenon of that peculiar form of respiration known as the *ascending and descending* is one of considerable interest, both practically and physiologically. It was first described by Dr. Cheyne in 1818, who observed it in a case of fatty degeneration of the heart, with steatomatous and earthy concretions in the aorta. Other examples of the same phenomenon were subsequently observed by Dr. Stokes, and it was considered by him to be pathognomonic, when present, of fatty degeneration of the heart.

Cases, however, have been lately recorded by Dr. Reid, of Belfast, Dr. Head, and Dr. J. Little, in which the phenomenon was present, yet in which (though either valvular lesions, or hypertrophy, or atheromatous disease of the aorta, or fatty degeneration of the diaphragm was present in all), no fatty degeneration of the muscular tissue of the heart existed.

Last year my friend Dr. Little read a paper before the Medical Society of the College of Physicians, in which he brought forward arguments to prove that the phenomenon in question is produced by a loss of balance between the efficiency of the two ventricles; whether that be due to fatty degeneration of the heart proceeding further in the left ventricle than in the right; or else to valvular disease, with or without aortic degeneration supervening so rapidly that the right ventricle has not had time to accommodate itself so as to work in harmony with the left. That, owing to the consequent insufficiency of the left ventricle for the work assigned to it by nature, arterial blood accumulates in the left auricle, in the pulmonary veins, and, finally, in the capillaries of the lungs, thus preventing the ultimate filaments of the pulmonary branches of the pneumogastric from receiving their customary impression from the black blood; which impression, being conveyed to the nervous centres, and converted there into motor force, is the chief cause of respiration. This impression, therefore, being absent, respiration ceases. But presently the gradual removal of the collected arterial blood leaves room for the advance of the black blood, and the consequent renewal of the impression on the pneumogastric, which is then followed by an act of respiration.

Such is Dr. Little's theory; and, though I believe it to embrace the true account of the essential mechanism of the phenomenon, yet, for certain reasons which I will presently mention, it seems to me, that in addition to the diseased conditions of the heart to which I have referred, a certain nervous complication is necessary to determine the accession of this peculiar form of respiration, and without which it would not occur. As an illustration of this complication I give the following outline of a case:—

E. M., a female, aged nineteen, was admitted last year under my care into the City of Dublin Hospital.

About seventeen months previous to her admission she suffered from a cold, and very soon after palpitations and cardiac distress began. When admitted into hospital there was considerable lividity of the features, especially of the lips and the extremity of the nose; and the veins of the forehead, chest, and arms were prominent and blue. There was some dyspnea, and the limbs were thin and wasted; no œdema anywhere. Corresponding to the base of the heart there was a slight systolic bruit, which was heard louder at the second *left* cartilage than at the second *right*, as if the bruit were situated in the pulmonary artery. The second sound was well pronounced and clear. At the apex a loud, slightly musical, systolic murmur was heard, and this was preceded by a slight presystolic murmur. This latter, however, was absent for several days before death. Tracing with the stethoscope to the right, towards the zyphoid cartilage, the murmur lost its musical character, and became harsh, but not diminished in intensity. Fremissement was very well marked at the apex. The pulse was exceedingly feeble, rapid, and intermitting.

The diagnosis was, therefore, constriction of the mitral orifice, tricuspid regurgitation, and, owing to the fact that the murmur at the base was more distinctly traceable towards the left than towards the right, I was inclined to believe there was disease of the pulmonary sigmoid valves also, though it is known to be such a very rare phenomenon.

The patient did well for exactly a fortnight, but at the end of that time, while endeavouring to get out of bed at night, she fell on the floor, and was with difficulty returned to bed again. Next morning I found her lying in a semi-comatose state. "Perfect" hemiplegia had taken place, sensation and the power of motion having been entirely lost on the left side. The pulse was almost

imperceptible, and very rapid, about 120. She was capable of being roused from this semi-comatose state, and then she exhibited some delirium and childish imbecility, and had considerable difficulty of articulation. The pupils were slightly contracted. The surface of the body and the extremities were cold, especially on the paralysed side.

But the most interesting feature of the case was, that now, for the first time, the respiration afforded an example, the best I have ever seen, of the ascending and descending form. There were intervals of perfect apnea, and these were almost exactly equal in duration to the periods of respiration, and each lasted for an almost constant period of fifteen seconds. Thus each minute was divided into four periods—two of perfect apnea, and two of puerile respiration. And, furthermore, an important and interesting fact is, that it was *only* while the patient was allowed to remain in the semi-comatose state in which I found her that this peculiar form of breathing was present. When she was roused up the respiration became almost normal, but again it assumed the ascending and descending character when she was allowed to lapse into the semi-comatose state. During the apneal period the pulse was more frequent, but more steady and more easily counted, than during the respiratory period.

This semi-comatose state, along with the peculiarity of breathing, continued very well marked for about twenty-four hours, and then together gradually became less distinct, and by the end of another twenty-four hours they had both entirely disappeared, and never again returned.

The patient lived for six days longer, the hemiplegia remaining "perfect." She then died.

Assisted by my colleague, Dr. Hewitt, I made the *post mortem* examination. There was a great deal of venous congestion about the face, neck, upper part of chest, and shoulders. On opening the thorax the lungs were found very much congested. At the apex of the left lung, and also about the middle of its lower lobe, we found a circumscribed apoplectic extravasation, in each case oval, and about one inch and a half in diameter; and the entire remaining portion of the apex presented a good example of diffused apoplexy. The veins on the outer surface of the pericardium were highly congested. When this bag was opened about an ounce and half of healthy serum was found contained, and the heart presented an unusually large appearance. In the left auricle a yellow

fibrinous clot was found, and the auricle itself was considerably dilated and hypertrophied. The left auriculo-ventricular opening was found to be contracted to a very small size. When the left ventricle was opened the mitral valve presented a well-marked funnel-shaped appearance, a condition so well described by Mr. Adams in the fourth volume of the "Dublin Hospital Reports." The flaps of the valve forming the funnel were greatly thickened, and hard, and as if welded together, and the mitral orifice at the apex of the funnel appeared as an elipse in shape, measuring about four lines in its major axis, and in its minor axis about two lines. The cordæ tendiniæ were absent. The musculi papillares, therefore, appeared to be inserted directly into the funnel. The walls of the ventricle were not hypertrophied, but its cavity was considerably diminished. The aortic valves were found to be slightly thickened, and about a line below the corpus arantii, on two of the flaps, was found adhering a small vegetation, about the size of a pin's head, sufficient to cause the murmur heard there. The caliber of the aorta was greatly reduced, and about an inch above the semi-lunar valves, the aorta, instead of preserving its natural curve towards the right sterno-clavicular articulation, became suddenly bent towards the left, at almost a right angle, thus accounting for the direction in which the systolic murmur at the base was most distinctly traceable during life.

On the right side of the heart the ventricle was found greatly dilated, and the tricuspid flaps considerably thickened and stiffened. The pulmonary sigmoid valves were quite healthy. The muscular tissues of the heart and diaphragm were perfectly free from fatty degeneration.

On examining the brain to ascertain the cause of the left hemiplegia, we found the whole brain rather flabby, but especially the right hemisphere. The lateral ventricles contained a few drops of slightly reddish fluid. The right corpus striatum was uniformly softened to a pap-like consistence, and embedded in its substance were three small extravasations of blood, extending from the size of a millet seed to that of a pea, and having the appearance as if several minute capillaries, in the immediate neighbourhood of each other, had simultaneously given way without producing any distinct clot.

This case, I think, goes far to prove that ascending and descending respiration is in great part a nervous phenomenon, though, for its production, it seems essential that there should be either fatty

degeneration of the heart, or constricted mitral orifice, or some cause tending to produce an accumulation of red blood at the left side of the pulmonary circulation, in fulfilment of the conditions considered necessary by Dr. Little.

That the ascending and descending respiration in this case was in great part a nervous phenomenon is borne out also by the fact, already mentioned, that it was *only* when the patient lay in the semi-comatose condition that it was present, whereas, when the patient was roused up, and the centres stimulated thereby to increased action, and also when the semi-coma had passed away permanently, respiration became almost normal.

In this case the extremely contracted mitral orifice, the diminished capacity of the left ventricle, the diminished caliber of the aorta and its great branches, and the very small pulse, indicate what a very scanty supply of nutrient arterial blood must have been sent to the brain in common with other organs. This was, doubtless, the cause of the softening, which had probably been proceeding gradually and insidiously for a considerable time, but which had not deprived the centres of their power of receiving and reflecting certain impressions. Consequently, as in health, the impressions produced by the black blood on the ultimate filaments of the pneumogastric were still received at the centres, and reflected back in the form of a motor impulse, which caused the natural respiration. When, however, the sudden disruption and disabling of a large proportion of the nervous fibres took place (which both caused the hemiplegia and permitted the fall out of bed), the centres were suddenly incapacitated for reflecting impressions with their previous energy. Respiration, therefore, ceased, producing the phenomenon of apnea. However, after a certain interval, during which the nervous centres were at rest, their excitability was so far restored that they became capable again of reflecting a motor impulse, and respiration was re-established. But owing to their weakened state the centres could not long sustain the effort, and another period of apnea succeeded; and so on.

Now, when we consider the number of cases of suddenly weakened nervous centres, unaccompanied with this peculiar form of respiration, on the one hand; and on the other, when we consider the number of cases of heart disease in which all the conditions conducive to a plethora of the pulmonary veins are present, and yet which are not accompanied with ascending and descending respiration; and when, again, we consider that, in the case before

us, this form of respiration was not produced by the diseased heart alone, and was only developed simultaneously with the manifestation of the nervous lesion; when, I say, we review these considerations in the light of the foregoing remarks, we cannot but believe that for its production a double pathological condition is necessary, viz.:—

1. A certain diseased state of the heart, by reason of which, indirectly, the excito-motor impulse upon the nervous centres, conveyed through the pulmonary branches of the pneumogastric is diminished.

2. A certain weakened state of those nervous centres, by reason of which the reflecto-motor impulse is diminished.

For were the heart alone diseased, and the centres perfectly sound, then, in obedience to the law of demand and supply in the animal economy, the excitability of the centres would doubtless become sufficiently increased (which it cannot when they are diseased) to compensate for the weakened impressions conveyed to them, and thus, the reflecto-motor impulse being undiminished, respiration would therefore remain continuous.

And were the centres alone weakened, and the heart sound, in this case, no accumulation of red blood at the left side of the pulmonary circulation could take place; so that in neither case, it would seem, could the phenomenon exist, since the conditions at present considered necessary for its production would be absent in each case singly, though present when both were combined. So far as I am aware no other record than mine has been made of the state of the brain in the cases of heart disease, in which this form of respiration was observed. Probably in every such case some imperfection of the nervous centres was present, though not apparent. In some of the cases there may have been no indication of its existence during life, as in my case there was none up to within a week of the patient's death.

I submit these considerations, hoping that they may induce others to observe narrowly cases presenting the form of respiration we have been dealing with, that, by an accumulation of accurately-recorded observations, we may at length arrive at a certain solution of this interesting and important question—important, because if my view be correct it involves two portions of the tripod of life.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Mind and Brain; or, the Correlations of Consciousness and Organization, systematically Investigated and Applied to Philosophy, Mental Science, and Practice. By THOMAS LAYCOCK, M.D., &c.; Professor of Practice of Medicine in the University of Edinburgh. London: Simpkin, Marshall, and Co. 2 vols. Pp. 919.

THE stationary condition of mental science, when contrasted with physical, has long been a reproach which every succeeding generation of metaphysicians has vainly attempted to remove; but it is futile to attempt to disclaim the imperfect and immature state of these branches of knowledge, as it is abundantly testified by the large number of schools of philosophy differing diametrically from each other often on the interpretation of some of the commonest phenomena of consciousness. From the extreme of Hæckelism, on the one hand, and the semi-materialism of Owen to the pure Idealism and the wild theories of the mystics, we have all grades of opinion on mental science wholly irreconcilable and at variance with each other. This being the case, a book which professes to bring this chaos of opinion to order, and which proposes to found a philosophical and physiological basis for mental science, is one which deserves notice; as, if this task be accomplished by its author, a desirable foundation will be laid upon which a more perfect superstructure of metaphysics can be reared than on the older and less perfect systems which divorced physiology from metaphysics. Dr. Laycock's book is not a new one; it has been before the public for some time, and the method in the edition just published is the same as that in the first; but as this, in a work on metaphysics, is of primary importance, it may be well to notice some of the leading features of the method proposed, and of the manner in which the plan is executed.

To justify the ambitious title which Professor Laycock has chosen for his book, very great research and thought have been

necessary; and our author has, in his two volumes, fully sustained the credit of the name, and has shown himself thoroughly conversant with the details both of mental and physical science; and while we cannot fully coincide with all the views therein expressed, yet we must congratulate Dr. Laycock on producing that rare thing, an interesting and readable book on metaphysics, and on leading the van in a most important and novel department of mental science—that which correlates its phenomena to those of matter and force. It would be impossible, in the course of a short review, to survey the entire work; it must suffice us to map out its general plan, dipping here and there whenever our author tempts us with a tit-bit of more than ordinary delicacy.

Beginning with the principle that the brain is the seat from which, in some way, the phenomena of mind emanate, he deduces that there must be a connexion between the physiology of the brain and the practical science of metaphysics; and that a knowledge of this connexion must be of importance in relation to the advancement of the happiness of individual man, the welfare of society, and the knowledge in which consists man's dominion over nature. The progress of mental philosophy so desirable he shows to be hindered by its present chaotic condition depending on the conflicting theories of first principles held by leading metaphysicians, the divorcing of physiology from philosophy, and the opposition of prejudiced theology, so frequently a source of error in scientific opinion, as well as from deficiencies in the methods of pursuing metaphysical investigation, which have obviously characterized the writings of many of our leading mental philosophers. Dr. Laycock purposes to pursue his investigations on the following plan. Starting from the generalizations derived from the common sense and experience of mankind deduced from language, laws, &c., he attempts to determine the first principles of a science of mind by observation and induction. This inquiry he divides into three heads—1st. That which relate to states of consciousness; 2ndly, That which relates to the fundamental laws of existence; 3rdly, The principles of mind as an ordering force in nature; in other words, psychology as deduced from experience, ontology, and teleology.

In discussing the principles of experience, our author rather hastily glances at some of the ideas and conditions of consciousness, arriving at conclusions similar to those which are familiar to students of Hamilton and Ferrier's works. He shows properly

that the ego, as a proper subject of psychological study, should be the ego concrete, not the ego abstract and immaterial, for truly is it a fundamental principle that existence precedes thought, and that the being existing is not necessarily the being conscious of existence, while its unity is not lost. This doctrine is more satisfactory than the principles of Descartes and Locke; that dentity consists in consciousness upon which Reid founds his celebrated paradox of the non-continuity of life in the officer who had forgotten the events of his boyhood.

Consciousness is then discussed in its relations, and the term is used in the sense indicated in the admirable lectures of Dr. Brown (p. 69); its relationship to belief, attention, and memory are indicated, and the relationships of conditions of consciousness to states of body pointed out. The question whether consciousness can be occupied with more than one object at the same time is considered; and Professor Laycock deduces the conclusion that the so-called simultaneous perception of multiple objects is truly successional; and we think, with Brown, Locke, Stewart, and many of the older writers, the phenomena of consciousness point to a remedy of perception; thus, in the six marbles of Hamilton we first get our idea of the collective group as a unity, and proceed successionally to analyse them, the entire process being truly not single but a train of linked perceptions.

Passing on to the consideration of ontology, our author devotes a chapter to the subject of unconscious existence, reviewing the subject alluded to before, of the continuity of consciousness; and in these he very properly regards the experiments of Sir William Hamilton on himself as utterly invalid to establish the theory of a doubly conscious existence; for it is in the experience of every one who has been awakened from sleep suddenly that in the act of awakening the mind often passes through long trains of thought whose origin we can find on reflection to be due to the very process of awakening, although they impress our memory as if of protracted duration. That consciousness may be latent, paradoxical as it may seem at first, is a point easy of demonstration, and one of practical moment; its consideration is made the subject of another chapter.

Passing on to the third department, or that of mental dynamics or teleology, he commences by a discussion of the correlations of causes—we first study the nature of law, its relation to force, to life; and finding the uniformity with which all matter is subject to force of one kind or another—living matter being under the

dominion of life and vital forces, and dead matter under the dominion of the physical and chemical laws of nature—our author from thence deduces that (the latter being universal forces and the former particular, or those which are apparent deviations from these, but which are in reality another form of manifestation of the universal forces), as all these forces are derivatives of another force or mind, therefore mind is the universal element in our cognition of both the physical and vital forces.

To bring out this point—the main point in his theory—our author carefully reviews the universally-accepted doctrine of the correlation of the physical forces and the more uncertain but equally attractive study of the correlation of the vital and physical forces finally advancing a step further, he attempts to correlate mental with vital or physical forces. If this be granted, and ideas taken as causes, it follows that as the idea shapes out the end to be accomplished, and only leaves variable the method of accomplishment, any mode of study of the causal relations of ideas must necessarily be teleological, and all secondary causal agencies must be the direct derivatives of, and be contained in, the first cause.

In considering and deciding the relations of mind in a causal point of view, we are led naturally to the consideration of the mind of Him who is the Great Designer; and the study of the characters of the Divine mind is fraught with new interest from the Biblical doctrine that man is made in the image of God; and there are two leading schools of philosophy which take opposite grounds on this very subject, the one regarding God as the Supreme Head and regulator of creation but not a part of creation; the other, or the Pantheistic school, regarding God as an ever-operating constituent principle in creation; this is the theory embodied in Brahminism and a leading feature in the extreme of Broad Churchism at the present day. Adopting the Biblical doctrine that man is made in the image of God, Pantheism is necessarily inadmissible, unless we deny our own personality. The intuitions of the largest portion of the human race are likewise in favour of a superintending personal deity; and this is the foundation of all the systems of worship which mankind have constructed for themselves in all ages. Besides, the idea of a central superintending origin of force is the natural induction from the gradational series of causes observable in nature, forming the limit that bounds our researches in every direction.

Having thus discussed the philosophy of causes, Dr. Laycock

takes up the second department of mental dynamics, or that of biology, and considers the conditions under which existence is possible, which are cause, matter, space, time, and change—conditions whose correlative conscious states are intuitions in the human mind; tracing these conditions, we have some very interesting notes on the probable genealogy of creation. Matter, first formed as a "rudis indigestaque moles," that which is the basis or חֹשֶׁךְ of the universe; into this was infused the energy or force by the Creator, for the "light" of Gen. i., 1, 2, is the fire of Ezek. v., 2; the heat of Isaiah xxiv., 16, the sun of Job xxxi.; the lightning of Job xxxvii., 3; in fact, is the primitive force—that which becomes the indestructible attribute of matter; and thus it is that Anaxagoras has stated, Νουν μεν αρχην κινησεως. Mind, the mind of the Creator, is the beginning of force; from His will the mechanism received its primitive impetus, and this impetus developed as a particular case of the universal force is thus causal of life.

The study of life from a philosophic standpoint as that of a correlated physical force requires a clear understanding of first principles, for the phenomena are ever changing in two directions—1st, with regard to time; and 2nd, with regard to space. The first group are consistent with the permanency of individuality and type, although attended with incessant change; and in connexion with this our author makes some interesting remarks on cyclical changes and on rhythmic action. The study of the second group opens to us some interesting questions. Form is the leading particular of existence in space, and the question naturally arises, What are the laws moulding or producing form? In matter form results from the equal balancing of forces. So, in the words of the ingenious author of the *First Lines of Science Simplified* (Dr. MacVicar, of Moffat), the forms of objects are their isodynamic boundaries in space, and thus bodies have separate, visible, magnetic, photic, or thermal forms; other qualities likewise depend upon isodynamism, as weight and consistence.

The inquiry as to what are the laws of absolute form leads us to distinguish two series of forms—typical and individual—the first being the result of the characteristic isodynamism of a class or group of forms; the second being the special isodynamic boundary in the individual, the archetypal or class typical form acted on by special disturbing influences. The primary form produced by the action of single forces in a single body is the spherical or circular.

Compounds of this primary are not composed at random, but are made up (in the archetypal forms) of harmonic curves, or curves subtending harmonic angles, and with radi in harmonical series. If the lines of force are equal in each half of the united body, lateral symmetry results; if in each fourth or smaller proportional a radiant symmetry arises, as in the cœlenterata; now, as growth under pressure or resistance causes a spiral extension, a condition illustrated in the embryonic development of the alimentary canal. Mr. Hinton, in his paper on "The Law of Organic Form" (*Brit. and For. Med. Chir. Rev.*, Oct., 1858), regards the numerous spiral and helicoid curves in nature as due to this cause.

Our author notices briefly the beautiful theory of Mr. Hay—a theory which deserves better treatment than it has received as yet, and one which we may predict will yet occupy a high place in æsthetical and practical science.

Finally, the teleiotic ideas being evolved not only as laws of form but as mental faculties, Dr. Laycock seeks here for the explanation of intuitive ideas. If we admit, as we cannot well avoid, that there are such things as intuitive ideas, their origin must naturally be shrouded in mystery; and hence any hypothesis which attempts to throw light on them deserves to be examined carefully. However, we think our author hurries into a corner, and slurs over a point of vast importance, and one which would have been better left out as scarcely coming within the scope of the book—we refer to the question of the origin of evil, a subject so awful and vast as to require, if noticed at all, a careful and cautious consideration.

Thus far for the first volume; highly commendable, interesting, and readable as it is, there are three points which have forcibly presented themselves to us in the course of a careful perusal—1st, That some parts of the book are rather obscure in phrase, and the multiplication of out-of-the-way and new words so favourite with the author, is rather to be deprecated. Metaphysical study is necessarily a difficult one, so it should be the aim of all writers to simplify their phraseology as far as may be consistent with accuracy of expression. Sentences like the following are numerous enough, and some of them require much digestion before the very simple contained truths can be assimilated:—

"The energy for verification with its correlative of doubting is developed in proportion to the scope of the fundamental ideas which correlate the intuitions, and the conviction of their truths."

This character, we fear, will prevent the book from filling the first requirement for which it was intended, viz., a student's class-book of mental science.

The second point, which we think is weak, is the want of demarcation between the two conceptions of mind and soul. Mind is, according to the author, the great central force—not capable of existing by itself; and hence the fault of metaphysical systems which do not take for their basis of study the concrete ego. But the great central force of divinity, the divine mind, does it exist independent of matter, or does it not? if it do, then it is a force which cannot be correlated to the forces of nature (*i.e.*, to the ordinary vital or physical energies); if not, then we are shut up to the veriest Pantheism.

Now, the difficulty here seems to be this, do we or do we not recognize a separate principle separate from life or energy—a soul? if not, then, of course, Dr. Laycock's arguments and doctrines are easily understood; but if we do, what then? Can the operations of a soul be correlated to those of energy in any form? A true separation of soul from mind or energy as depending on matter or material conditions would solve this and other difficulties in the theory. This confusion of idea is the more to be regretted as it is obviously unintentional; for Dr. Laycock proves that he believes in the immortality of the soul and the existence of an immaterial Deity; and the obscurity arises from the want of definition of the expressions *mental*, *mind*, and their synonyms, without a sufficiently definite limitation.

The third point is, in defining matter and mind the common expression is referred to as embodied in language. Matter, we are told, is not mind, and *vice versâ;* but the common experience is not necessarily scientifically correct, nay, is eminently incorrect; for, in popular phrase, the electric fluid is characterized as if a ponderable, and heat is often regarded as an entity; but these are both forms of the great central force correlated to life, therefore experience is at fault in regard to them. This is noticed because there is a little laxity in the definition.

Leaving these flaws—and they are singularly few considering the immense scope of this part of the work—it is wonderful what an amount of solid substantial thought is embodied in a practical come-at-able form in this volume.

The second volume opens with an attempt to found the principles of a scientific psychology. Now, to do this it is

necessary to determine what is the substratum of mind in which the various feelings and mental phenomena are shown; and here the difficulty noticed before starts up to view, for the fundamental proposition states "that mind, as the first cause, is not only inseparably associated with the primary forces of matter, but that it acts in and by those forces alone. The results of these forces are its exponents; their phenomena are its signs and language, the expression of its thoughts." This statement is pregnant with the difficulty that if it be true there is no energizing without matter, therefore if a God existed before matter He was a Being incapable of being a centre of causation, and hence no God at all. If mind here means that form of energy impressed first of all upon matter the central physical force which receives its highest exaltation in intellect (as distinguished from soul), then it is probably true and does form a most important basal proposition in scientific psychology.

Having determined that mind (as distinguished from soul) and mental conditions are correlatives of the physical forces and closely associated with matter, it follows that as physical forces are manifested by change in anorganic and merorganic bodies (Lewes), so mental change is correlated to teleorganic changes. The substrata of matter in which changes producing mental states exist he designates ideagenic; those in which changes producing motion exists are called kinetic; and with reference to the former Dr. Laycock makes the following remarks:—

"Thus each primordial or embryonic cell has its psychal substrata, in virtue of which there is a continuous series of successional changes in a fixed, predetermined order, and according to a fixed plan. So also in these cell-masses (or vesicular nervine) appropriate to special ideas there are psychal substrata in virtue of which there is a constant construction of new cells corresponding to those new states of consciousness comprised under the general term development of ideas, the ideas being developed and new cells constructed according to a fixed and predetermined law of development."

The special meaning of this we do not profess to understand, so we pass on to the next chapters, which deal with the correlations of life and consciousness, with special references to the laws of vital action, pleasure, pain, titillation, and desire, after which the subject of the evolution of new instincts genetically and their hereditary transmission is brought under consideration, and numerous illustrations from natural and ethnic history are given of it.

The general doctrines of the faculties, feelings, and beliefs, as derived from experience, are then passed in review; and here we touch upon a point which requires careful investigation, viz., the classification of the faculties, a point of importance in the study of phrenology.

In attempting to found a new system of mental philosophy upon the basis of the primary fundamental theory of the correlation of mental and physical force it is necessary to trace systematically the evolution of the mental phenomena from their simplest conditions to their most complex forms.

The first of these brain functions is that which Professor Laycock designates its vivifying function—or that by which the man is self-conscious and acts as one with the external world, and by which the animal and organic functions are made to coincide, the centre which correlates the features, gait, and expressions of the individual. As an illustration of this, but not a very happy one, Dr. Laycock refers to the social polyps and polyzoans, and describes at length the structure of a hydrozoan polypary with regard to its community of nutrition; but how this "view enables us to comprehend the true character of the encephalon as the organ of mind" is not very clear, as the analogy is rather strained.

A knowledge of the structure of the encephalon is, of course, a natural requirement in the construction of this system of philosophy, and so is an inquiry into the physical properties of brain matter. That these physical properties have no relation to the vital tendencies we can scarcely predicate; for, as the physical properties are the results of force, and as mind is correlated to this force, we might expect that the physical properties would in some sense be related to the vital affinities of the substance; and hence the interest in the researches on the relation between molecular composition and physiological action. In connexion with this, our author lays the ghost of an objection which has been often brought against phrenology, that as there is no line of demarcation between the various parts of the grey matter, therefore there can be no specialty in function. Now, this objection is, in our mind, thoroughly invalid; but one of the Doctor's methods of dealing with it is certainly very inconclusive—we refer to the analogy drawn by him between the digestion of the polype and that of the higher animal; the various cerebral cell actions, although differing in degree, are the same in kind; and hence (to use the Doctor's own words) there is no necessity for a differentiation in the

ideagenic substrata of the brain. This is not the real ground of discussion with the phrenologist, as we shall see.

The forms of heresy most difficult to deal with are those which contain a basis of truth, and physiognomy is one of these; that under any system of investigation it can ever be brought to rank as a science cannot be believed; and hence, were it possible even to have a thorough acquaintance with the two requirements necessary for this elevation, not much advance in this direction would be gained. "A knowledge of two fundamental but distinct laws of life and organization is requisite to raise phrenological diagnosis to the rank of a science—namely, first, of the archetypal law of facial and cranial development in the various races of men and vertebrates generally; and secondly, the law of modification of this archetypal law under the influence of the cerebral hemispheres and the cerebro-spinal centres."

Dr. Laycock gives a historical sketch of phrenology and of the arguments against it, but writes with a strong bias in its favour, and passes on to discuss the classification of the appetites and emotions. There are many interesting points touched upon in this, but the professor exhibits the usual phrenological tendency to regard each as specially circumscribed—a tendency in which he will not be followed or coincided with by many metaphysicians.

The last portion of the book is devoted to the relationships of cerebral organology, and in it the structure and functions of the nervous system are sketched out. We cannot see why he or others regard the nerve ganglia in Ascidia and Phallusia as the homologue of the sympathetic, while the ganglia in Lamellibranchs are supposed to represent the cerebro-spinal system (p. 421). Further on the topographical distribution of convolutions is considered, and their supposed phrenological bearings are suggested.

In a work of such enormous scope, which is nothing short of a "philosophy of the universe," the number and variety of the subjects discussed preclude the possibility of a complete and exhaustive criticism; the whole work, however, stands or falls with the truth of one proposition—that mental force is a correlative of vital force and vital of physical force. If this be true the positions of the book are good; if false the metaphysical system of Dr. Laycock is also false and untenable.

Contributions a la Chirurgie. Par M. le Docteur CH. SÉDILLOT, Médicin Inspecteur des Armées; Directeur de l'Ecole Impériale du Service de Santé Militaire; Professeur de Clinique Chirurgicale à la Faculté de Médicine de Strasbourg, &c., &c. 2 Vols. Paris: 1868.

UNDER the title of Contributions to Surgery, Dr. Sédillot has published a collection of original memoirs on subjects to which he has particularly directed his attention from the early part of his professional life to the present day. He disclaims any intention of publishing this collection as a systematic treatise on surgery; he publishes it as a review of his own surgical career, which has extended over a period of nearly forty years. During this time hardly a year has passed without the publication of some memoir by him. One of these, not included in the present volumes, was noticed in the number of this Journal published last February; another on purulent infection, published in 1849, is well known to many of our readers. The present volumes contain papers on a great variety of subjects, which have been published from time to time in continental journals difficult of access to many of our readers, and which would be in consequence unknown to them had they not been reproduced in a collected form. Many of these papers, treating of the same subjects at different times, are most valuable as records of the progress of special branches of surgery, drawn up by one who has had ample opportunities of testing the value of the various modes of treatment proposed, and who has in all cases published the facts on which his judgment has been founded. Our author, apologizing for the absence of systematic arrangement, and for the unavoidable repetitions which occur in his work, states well the advantages of such a series of observations by asking his readers the following question:—"Is it without interest to follow an author in the development and progress of his ideas, to see the truth, at first hesitating, and, as it were, doubtful, affirm itself more and more, to see it triumph over opposition and impose itself as general doctrine?"

In the space which we can afford to the examination of this work, containing 1,369 pages, it would be impossible to examine in detail all its parts; we can only notice the more important and more recent parts, and recommend our readers to study the details for themselves, assuring them that they will find much practical and original information in every part of the work.

The first volume opens with an article on "Accidents Infectieux," as the principal cause of the mortality consequent on surgical operations. This article expresses the opinion of the author, which has been the subject of a special paper noticed above, and which runs through the entire of the present volumes—namely, that pyemia and the conditions allied to, or identical with it, are the most important causes of mortality after surgical operations or after injuries which fall into the hands of the surgeon to treat. He divides the accidents attending wounds into direct or immediate, intercurrent infections, and what for want of a better name we call fatal; the direct include hemorrhage, prolonged suppuration, wasting, delirium, entrance of air into the veins, syncope, asphyxia, &c. He holds that surgery can combat such with a comparatively small loss of life. The intercurrent include pneumonia, pleurisy, small-pox, scarlatina, &c.; these, too, he holds to be but a feeble cause of mortality. The infectious accidents he holds to be the true cause of the mortality attending ordinary surgery. Under this head he places pyemia, septic, putrid or gangrenous poisonings, or intoxications; with these phlebitis, inflammation of lymphatics, erysipelas, &c.

His last group of causes of mortality, which we have called above fatal, includes, as the author says, "fortunately" only great assemblies of men, armies in field, hospital, or prison, and he seems to separate these from those immediately preceding, because they almost entirely annihilate the resources of surgical art, not because of any absolute difference in their kind.

Our author, true to the maxim that the best treatment consists in obviating the tendency to death, in the many operative proceedings which he recommends, makes every effort to avoid what he regards as the principal causes of "infectious" accidents—namely, the tension of the soft parts, capable of causing arrest of the circulation, and the retention in the wounds of fluids liable to decompose, such as blood, &c.

The second chapter of the first volume contains a series of papers on the use of ether and chloroform as anesthetics; these papers are a complete history of this branch of surgery, and contain, in addition, much of value to the medical jurist. Dr. Sédillot adopts the words of Velpeau to express the advantages of chloroform over ether as an anesthetic:—"Chloroform acts more quickly, more surely, and gives a calm and a sleep more complete." He points out one fact of great importance, proved by experiment on

animals, that the effects of chloroform on the system last long after the administration of the vapour has ceased, to such a degree as to be fatal, notwithstanding free access of air and of the continuation of the respiratory and cardiac movements: it is necessary, therefore, to interrupt the inspiration of the vapour before complete muscular relaxation takes place, so as to prevent the dangers of an over-dose. In all his papers on the use of chloroform Dr. Sédillot maintains three propositions with reference to the administration, viz.:—

"1st.—Every time that the surgeon has recourse to chloroform the question of life or death arises.

"2nd.—The administration of chloroform is an art which requires attention at every instant, as well as much skill and experience.

"3rd.—Chloroform pure and well administered is never fatal."

His last article on the use of chloroform is dated 1867, and therefore contains the experience of the author from its first introduction into practice in 1847 to within a year of the present time; in this he gives what he considers the most important of the rules for its administration. They are as follow:—

"1. It is prudent to avoid too deep inspirations of chloroform, such as often follow cough or momentary arrest of respiration, which result from laryngeal spasm; such inspirations may be dangerous from the quantity of vapour inspired.

"2. Apparent regularity of the respiratory movements is no proof of entry of air into the chest, as the glottis may be closed; hence it is necessary to ascertain the existence of the respiratory murmur.

"3. Cough is a proof of the persistence of the irritability of nerves and muscles, and may be arrested by new inhalations of chloroform.

"4. Vomiting is equally a proof of incomplete anesthesia, and is capable of arrest by continued inhalations.

"5. It is very dangerous to attempt to paralyse completely the muscles in order to facilitate the reduction of dislocations, for their contractile power is roused under the influence of the extension of the limbs, whatever be the degree of relaxation previous to the commencement of extension, and notwithstanding the most profound general prostration.

"6. Ophthalmic operations require the same precautions as the reduction of dislocations, as a very complete condition of anesthesia must be reached to paralyse even for a moment the muscles of the eye, a condition which can not with safety be maintained.

"7. Paralysis of the tongue should be avoided, for the double reason

that its presence shows that the anesthesia has been carried too far, and from its tendency to produce asphyxia by the falling of the tongue backwards on the opening of the larynx.

"8. The indications for the momentary interruption of the inhalations of chloroform are principally, the quantity inspired, the appearance of the stage of excitement which is the constant indication of too rapid administration of the vapour, the state of silence, insensibility of the surface of the body, the upturning of the eye, and the commencement of the relaxation of the limbs."

Dr. Sédillot does not lay down these as a complete code of rules for the administration of chloroform; such he has given in detail in several parts of the papers comprising this chapter, and they are well known to all surgeons of the present day, so that we need not notice them here. Some of our readers will perhaps think those given out of place in this Journal, but they will find it no easy matter to discover in our best text-books any directions as clear in reasoning or statement. In the present day the administration of chloroform is a matter of such every-day occurrence in both public and private practice, that our teachers rarely think of training students to observe its details; and indeed they generally have other matters to deal with and to teach at the times of its use, no matter how frequent. Hence it happens that many students in the present day, who have not had the benefit of being interns in hospital, go forth to practise their profession, familiar indeed with chloroform, but quite ignorant how to administer it with that confidence which our author shows, when he says: "Chloroform pure and skilfully administered is never fatal."

Dr. Sédillot's chapter on dislocations contains a valuable series of papers on the use of the pullies as a means of reduction, on the superiority of position as a means of retaining joints in position after reduction, to the application of bandages and compresses, besides most complete papers on the determination and pathological anatomy of dislocations of the shoulder, hip, elbow, &c. He claims the merit of having pointed out the advantages of the pullies in the year 1835, and of having established their use in opposition to the leading surgeons of France at that date—namely, Boyer, Dupuytren, Roux, Lisfranc, and Desault. He insisted then on the necessity of guarding their use by employing the dynamometer to estimate the force employed, an instrument rarely used or even mentioned in our modern books. He shows from the results of his practice that a power equal to from 150 to 190

kilogrammes, about 330 lbs. to 428 lbs., is sufficient for the reduction of any dislocation, a much lower power sufficing in most cases. He holds that any dislocation which resists a traction carefully applied, equal to 190 kilogrammes, should be declared irreducible in the present state of science; that, in fact, it is not justifiable to carry the traction beyond this point.

He advises the abolition of the use of the bandages and compresses, so commonly placed around joints after the reduction of dislocations, in order to prevent the recurrence of the displacement, as these are unnecessary and frequently are the means of producing inflammation, ulceration, gangrene, permanent immolity, &c. Compresses, &c., he holds to be in almost all cases incompetent to prevent recurrence of a dislocation where this tendency exists, and their presence serves in such cases merely to conceal the accident of recurrence. He holds that the simple placing of the joint in a position the opposite of that in which it was when the dislocation occurred is sufficient to prevent recurrence; for instance, that the bending of the elbow is sufficient to maintain a dislocation of the radius and ulna backwards in its place after reduction.

Dr. Sédillot claims to have been the first to point with accuracy the signs of dislocation of the head of the humerus backwards or into the infra-spinous fossa of the scapula. He records a case of this dislocation reduced by him a year and five days after the occurrence of the accident by means of the pullies, the details of which will be read with interest by those of our readers specially occupied with the subject of rare dislocations.

The following is the classification of dislocations of the shoulder-joint proposed by Dr. Sédillot:—

"First form:—Dislocations forwards; varieties seven:—
"1. Partial or incomplete.
"2. Subcoracoid.
"3. Axillary.
"4. Subscapular.
"5. Intercostal.
"6. Scapulo-clavicular.
"7. Costo-clavicular.

"Second form:—Dislocations backwards; two varieties:—
"1. Immediate, or beneath the acromion and spine of the scapula.
"2. Mediate, beneath the spine of the scapula."

The author holds the opinion that before the head of the humerus can escape from the glenoid cavity it must be depressed, so as to be in contact only with the two inferior thirds of that cavity, in order to escape either forwards or backwards beneath the coracoid or acromion processes respectively. He holds it impossible for the head of the bone to escape directly downwards in consequence of the arrangement of the capsule and muscles around the joint, the insertion of the long portion of the triceps muscle and the oblique position of the bone at the time of the occurrence of dislocation. He attributes the confusion which exists in the description of the dislocations of the shoulder to the absence of definition of the terms and to the erroneous system of describing the glenoid cavity as a space bounded by four lines—superior, inferior, lateral or external, and internal. The second, third, and fourth dislocations in this classification resemble each other in many points, but differ in actual position of the head of the bone; in the sub-coracoid the head of the bone is in contact with the inferior surface of the coracoid process, and the anterior border of the glenoid cavity; in the axillary dislocation the head of the bone is still in contact with the neck of the scapula, but is remote from the coracoid process; in the sub-scapular variety the head of the bone lies deep in the sub-scapular fossa beneath the muscle. The intercostal luxation is introduced into the classification merely to include a single case recorded by Larrey. The names of the two last varieties define sufficiently the position of the bone. With reference to the first or partial variety, Dr. Sédillot holds that it is not possible to distinguish between it and the sub-coracoid in old-standing cases, for the changes induced by absorption, &c., reduce the sub-coracoid, to the condition of the partial. The author admits four varieties of incomplete luxation—1st, Primitive or recent, the possibility of the existence of which he doubts; 2nd, Dependent on relaxation of the muscles; 3rd, Consecutive to the reduction of sub-coracoid luxation; and 4th, The result of the changes produced by an old sub-coracoid luxation, the most frequent. Subsequent to the publication of this classification the author records an example of the rare form of dislocation not included in it, in which the humerus is displaced directly downward on the border of the scapula, and details the *post mortem* examination of the case.

The dislocations of the elbow, which Dr. Sédillot examines in his chapter on dislocations, are that of the ulna backwards without displacement of the radius, of which he has collected five cases; and

dislocation of both bones of the forearm backwards, mistaken for a fracture, and reduced after 114 days by means of the pullies. The signs which he enumerates as characteristic of the dislocation of the ulna by itself are: pain following a fall on the wrist, impossibility of the movements of flexion and extension of the joint, while the motions of pronation and supination exist; shortening of the ulnar side of the forearm, with inversion of the forearm and hand; an abnormal direction of the forearm inwards; a change in the form of the elbow, the external side presenting an angular protrusion, the internal a corresponding re-entrant angle; an augmentation in the circumference of the joint; and a projection on the inner side of the joint formed of the humerus and its epitrochlea. His examination in detail of each of these symptoms will well repay such of our readers as are interested in this rare form of luxation.

In the year 1861 Dr. Sédillot published a paper on dislocations of the femur into the obturator foramen, which we think the most important for us to examine in this short review of his work, referring such of our readers as are interested in the minute details of congenital and other dislocations of the thigh, to his work for more complete information. We select this paper for examination, as it contains two facts of great importance with reference to this special form of dislocation. The first of these facts is the personal experience of Dr. Sédillot as to the relative frequency of the different forms of dislocation of the thigh. The general opinion of surgeons is that dislocations of the femur on the dorsum ilii are much the most common as the result of injury. The experience of Dr. Sédillot is different from this; he states that from his personal experience he is authorized in holding with Boyer and Hippocrates that dislocations into the obturator foramen are the most frequent; and it has fallen to his lot to reduce six dislocations of this kind, a number far exceeding that of any other variety. The second fact, and that which is of most importance in this communication, is that patients suffering from dislocations of the femur into the obturator foramen may retain the power of walking with facility from the time of the occurrence of accident; and hence it is very possible that such dislocation may be overlooked. Dr. Sédillot records six cases in which this preservation of the power of walking immediately after the injury was observed, in two of which it seems probable that the injury escaped notice in consequence of this very fact. We think that Dr. Sédillot's authority will be accepted as proof of the nature of the injuries in these cases, and we agree with him in the importance of their publication.

In the preface of his fourth chapter, which is devoted to the consideration of fractures, Dr. Sédillot dissents from the directions of Sir A. Cooper as to the treatment of intra-capsular fractures of the neck of the thigh bone in old people, maintaining that in all cases the treatment should be conducted with a view of obtaining bony union of the fracture. The grounds on which he founds his opinion are that he has never found the requisite treatment so injurious to the patient as it is generally stated to be; and secondly, that he has seen bony consolidation the most deformed occur in cases when the treatment has been neglected. We cannot coincide with Dr. Sédillot in the few remarks which he has made on this subject; his paper which contains them is not dated, but as it is a preface to the chapter, we conclude that it has been written but recently; if so, its shortcomings are all the more surprising, for in the other portions of the work Dr. Sédillot shows himself to be well informed and accurate in his observations. He can hardly have seen osseous consolidations the most deformed produced in intra-capsular fractures, for bony union rarely, if at all, occurs in intra-capsular fracture of the femur, except in the impacted variety, and the recorded cases of such union, sufficiently rare, do not exhibit such deformity as he describes. We suspect the explanation of this statement is to be found in the sentence that follows it:—

"The differential signs of intra and extra-capsular fractures of the neck of the thigh bone are for the most part arbitrary and uncertain."

We know that extra-capsular fractures unite in many cases with great deformity, the result of the great deposits of bone formed around the detached trochanters and neck, and the displacement of the trochanters and neck themselves, and that such deformity is but little under the control of the surgeon; and such, we think, must be the cases indicated in those remarks of Dr. Sédillot. We do not take exception to his treatment, if his experience proves it to be successful; but we cannot agree with him in his reasoning, and we consider that the above statements justify us in concluding that he has failed to distinguish between the two forms of fracture.

Dr. Sédillot proposes a new method of treatment for fractures of the lower end of the radius, which has the merit of simplicity and of being based on reasoning which appears sound. He states that he has adopted this with marked success for sixteen years. The reasoning on which he bases his treatment is, that in the movements

of the lower end of the radius around the ulna, the extremity of the styloid process of the radius constantly descends as the bone passes from a state of pronation to extreme supination; this descent of the styloid process increases the length of the forearm measured from either coronoid or olecranon process of the ulna to the point of the styloid process of the radius by from five to ten millemetres, or from $\frac{1}{5}$ to $\frac{2}{5}$ of an inch. Dr. Sédillot observes that the chief loss of motion in fractures of the lower end of the radius treated in a prone position or abandoned to themselves, is a limitation of the motion of supination caused by the union in a bad position altering the relations of the styloid processes of the radius, and giving rise to shortening of the anterior carpal ligaments and of the pronator quadratus muscle. His treatment consists in maintaining the forearm in extreme supination by splints, or what he thinks the most efficient means, by a starch bandage. He recognizes the difficulty of maintaining the supine position efficiently, and points out the necessity of keeping the forearm in position during the drying of the starch bandage.

Dr. Sédillot's eighth chapter treats of a disease which has of late years been the subject of several memoirs by continental surgeons—namely, the perforating disease or ulcer of the foot. In the year 1852 M. Nelaton described, in the *Gazette des Hôpitaux*, "a singular disease of the bones of the foot;" and in the same journal, one month after M. Nelaton's paper, a similar case was published by M. Vesignie, and the disease named the "perforating plantar malady." In 1855 M. Leplat proposed, in a thesis on this disease, to name it "the perforating disease," omitting the term plantar in consequence of the fact of the disease occurring in various parts of the body. M. Richard has described a "perforating disease of the leg;" and M. H. Bertrand published, in 1865, a case of perforating disease of both hands and feet, and proposed to name it "the perforating disease of the extremities." Dr. Sédillot denies the recent origin of this disease, maintained by the authors mentioned above. He holds that a description of an affection possessing all the characters attributed to it by them are to be found in a lecture of M. J. Cloquet, published in 1837; again in the *Traité des Maladies Chirurgicales* of Boyer, and again under the title of Warty Ulcer in Margolin's Dictionary. Dr. Sédillot publishes two cases of this affection in detail, and states his opinion that the affection is simply an ulcer, the tendency of which is to progress, habitually attacking the centre of epidermic thickenings; it has a great tendency to

relapse, while its rate of progress is liable to much modification by individual conditions. The cause of the disease he holds to be pressure of the soft parts between the ground and the bones of the foot.

The characters of the ulceration are thus described in one of our author's cases:—

"An ulcer placed on an induration beneath the head of the fifth metatarsal bone, which has existed for some years, the opening circular, more than the size of a franc piece, apparently not passing deeper than the skin, the epidermis much thickened around the borders, which are vertically cut, the depth of the ulcer occupied by flabby grey material resembling dead cellular tissue. No pain is caused by touching or probing this ulcer; and the probe, passed into its deeper part, touches the bone, bare and uneven, but not softened."

While this form of ulceration has been most frequently seen on the metatarsal side of the arch of the foot, it has also been observed on the heel.

Dr. Sédillot's second volume contains papers on the arrest of hemorrhage, ligature of arteries, amputations, resections, separate chapters on the surgery of the mucous tracts, and on plastic operations. Our space will admit of the examination of but few of these subjects.

His paper in the chapter on amputations, entitled, "The Means of insuring Success in Amputations," is, we think, the most important, as it contains the statement of the principles on which the author has conducted his numerous operations; and the results of these, published for a sufficient length of time to enable us to judge of the soundness of the principles by the best test, that of their effect on practice. The following table shows the results obtained by Dr. Sédillot during the years 1848-49-50-51:—

	Cases.	Cured.	Died.
Amputations of thigh, . .	7	7	0
„ leg, . .	14	12	2
„ arm, . .	4	4	0
„ forearm, . .	1	1	0
„ foot (entire), .	1	1	0
„ foot (partial), .	2	1	1
„ carpo-metacarpal and metacarpo-phalangeal,	5	5	0
„ phalanges, . .	5	4	1
	40	36	4

These statistics show an amount of success rarely attained in the graver forms of operation, and naturally induce us to examine the details of the author's method of conducting amputation. He attributes much of the ordinary mortality to the strangulation of the stump by the conventional mode of dressing, œdema, swelling, obstruction of the circulation, and retention of serum, blood, and pus, &c., being caused by the pressure of bandages and sutures multiplied to excess, and so arranged as to be difficult of removal until drowned by suppuration. Such dressings are injurious by concealing the state of the parts and rendering their removal difficult and painful. Dr. Sédillot holds dressings to be unnecessary for the maintaining of surfaces of amputation in position, for he maintains that these surfaces should be such and should be so placed as to be in apposition naturally. To obtain this result the author has abandoned the circular for flap amputations, having recourse to a single anterior flap comprising the anterior two-thirds of the circumference of the limb, the posterior third being cut perpendicularly from the angles of the flap. So made the flap falls directly over the divided bone, from which in some cases it is advisable to protect it by a compress capable of being removed through the lower part of the wound, and serving as a guide for the escape of fluids. A couple of sutures suffice to hold the angles of the flap in place, and the ligatures are brought out with the central compress. The stump should be raised before suppuration, lowered when this has taken place. This description applies especially to the thigh, but to adapt it to other parts the author defines the sense in which he uses the term anterior to be that aspect of a limb which is uppermost in its natural position as a man lies horizontally. Dr. Sédillot has so modified the position of flaps in the ordinary amputations as to suit them to the position indicated by this description. In the mediotarsal operation he takes his flap from the inside of the foot; in the leg a single external flap, &c. The minute details of these operations, as described by the author, will well repay the student of operative surgery. The author's chapter on the surgery of the urinary organs contains the histories of many remarkable cases of obstruction of the urethra and of rupture of it. His discussion of the various methods of treatment of stricture show him to be in a singular degree free from prejudice in the determination of the best modes of treatment. He seems to have tried most varieties of treatment patiently and most with success; and he has been

throughout ready to examine any new method and adopt it if found of service. The following are his conclusions as to the forms of treatment which are indicated in the various cases of stricture of the urethra:—

"1. Dilatation in simple cases, either gradual or suddenly, effected by dilators.

"2. Internal urethrotomy whenever the stricture is complicated, whether inflammatory or traumatic, when it is not permanently dilatable, though permeable.

"3. Perineal urethrotomy where the attempts at re-establishing the urinary passage do not suffice to remove complications, and that sloughing and infiltration of the perineum demand incision.

"4. Lastly, the same operation in the cases, fortunately rare, where the obliteration or changes in the course of the canal render the introduction of a filiform bougie into the bladder impossible."

In the performance of internal urethrotomy the author adopts the method and instruments of Maisonneuve.

The chapter on the surgery of the gastro-intestinal tract contains an elaborate treatise on gastrotomy and the causes, which may require its performance; also a remarkable case of artificial anus with four intestinal openings, besides much other matter. The author has come to the conclusion that operations for the radical cure of hernia are too dangerous in practice, although they may give rise to some good results. He has therefore resolved to abandon them.

Dr. Sédillot's description of the operation for cleft palate will be read by all surgeons with great interest; it contains the history of the various methods from the earliest efforts to the present time, and gives the fullest details of the author's operations. His last remarks on this subject contain the statement that Langenbeck's method of attempting to form a bony palate by dissection of periosteal flaps, while it is a great success in the closure of the fissure of the hard palate, is, in his opinion, a failure so far as the production of new bone; it is, however, free from the danger of causing death of the denuded bone, which speedily produces a new periosteum. Our space does not permit of a more detailed examination of these truly magnificent contributions to surgical literature; but we trust we have so far exposed the character of the work and the range of subjects of which it treats, as to induce our readers to examine it for themselves. We cannot conclude our remarks on this book without noticing the favourable contrast which it presents

to the greater number of continental publications of its class in both paper and type. In these respects it is perfect; and though but sparingly illustrated, the woodcuts inserted in the text are most admirably executed.

A Treatise on Syphilis. By WALTER J. COULSON, F.R.C.S., Surgeon to the Lock Hospital, and to St. Peter's Hospital for Stone and Urinary Diseases. London: John Churchill and Sons. 1868. 8vo, pp. 373.

IN reviewing the works of Barton, Hill, and others, in the February number of this journal, the subject of syphilis was so freely discussed that we can do no more now than draw attention to Mr. Coulson's labours in the same field, supporting, as they do, the conclusions we then advocated regarding the essential distinction which exists between the three forms of venereal disease.

The author has unfortunately followed the example of some English medical writers who have preceded him, in ignoring the labours of Irish physicians and surgeons. In the chapter upon syphilitic affections of the nervous system, cases from the works of Todd, Trousseau, Zambaco, Roberts, Jackson, and others, are mentioned, and their labours acknowledged, while the Irish surgeon, who was the first to recognize the true origin of the symptoms in this important class of affections, is passed by in contemptuous silence. It is very generally known that Dr. Thomas Reade, of Belfast, published his first paper upon syphilitic disease of the nervous system in the pages of this journal in the year 1852; Dr. Todd, of London, having in a clinical lecture drawn attention to the same subject in 1851. All, however, are not aware of the fact, which we are in a position to vouch, that Dr. Reade's paper was offered for publication in the year 1847. Thus Dr. Reade was really and literally the first who recognized the existence of syphilitic disease of the nervous system, and his labours since this time in further elucidation of the same subject are well known, and should be freely acknowledged. While we regret this omission in Mr. Coulson's work, in which, as we have already said, he but follows the example of some other London surgeons, we ourselves are glad of the opportunity thus afforded us of placing in its true position the original observations of an Irish surgeon, the correctness of whose conclusions has been now fully established.

Lectures on Surgery. By JAMES SPENCE, F.R.S.E., Surgeon to the Queen in Scotland; President of the Royal College of Surgeons, Edinburgh; Professor of Surgery in the University of Edinburgh; Surgeon to the Royal Infirmary and Lock Hospital, &c., &c.

THE first part only of these lectures has been published, the remaining and more practical portions not being ready for publication in time for the commencement of the winter session in Edinburgh. We learn this fact from the publishers' introductory notice, and we mention it here as it shows that most probably the work has been hastily compiled.

The position which Mr. Spence occupies, as indicated by the title page of the work, leads us to expect that his lectures on surgery would contain much valuable and practical information. Our expectations derived from the title page are, however, far from being reached by the volume of lectures before us. They are wanting in arrangement, and contain many terms the use of which we cannot but disapprove. There occur in the first lecture the following sentences:—

"The blood discs or corpuscles, of which there are two kinds, the yellow and white." "We see that the yellow blood corpuscles move rapidly along in the central part of the vessel, while on either side and close to its walls is a clear space filled with liquor sanguinis, with here and there a white corpuscle moving much more slowly than the yellow corpuscles do."

In these passages the term yellow, which we have not before seen used in describing the blood corpuscles, is introduced without any reason being assigned for the innovation. We presume the author's intention is to convey to his readers the idea that his description is taken from what is seen in the field of the microscope, where certainly the faintness of the red colour of the blood globules might be mistaken for yellow; but this idea is not consistent with the term white applied to the second form of blood globule; for by the mode of examination, by transmitted light, necessary to show the colour of the first form, the second appears colourless.

We should not object to his using the term colourless in the above passages, for the term is in quite as common use as that of white. Under any circumstances we should object to the term yellow, without good reason being given for its use, as an innovation;

but we object to it in these passages because it appears to be used to convey the idea that the author is describing appearances as seen in nature, with which idea the word white is inadmissible.

In several parts of this volume a term occurs, which it may be easy enough to make out the meaning of, but which cannot be called correct, as in the following passage:—"Dress the part afterwards with warm water lint." This term warm water lint means lint wetted with warm water; not as we might interpret it, lint made with warm water, or made of it, if such were possible.

Mr. Spence thus describes the characters of malignant growths, so far as they are ascertained by manipulation:—

"On manipulation they feel soft and elastic, with here and there hard resistent portions; although in some parts of such a tumour the elasticity may amount to a feeling of fluctuation, or actual fluctuation may be present, yet in no case is it equal throughout. In this they present a contrast to the simple fatty tumour and the chronic abscess, the former of which presents a feeling of elasticity and the latter of fluctuation, but in each the feeling is uniform in every part."

Such is the description of the result of the manipulation of malignant tumours. But we find on the next page in the remainder of the rather meagre account given of the characters of malignant disease, a description of hard cancer, of tumours of fibro-cartilaginous hardness, &c. The passage quoted above must be intended as a description of some one form of malignant disease, and the context proves that the author is not ignorant of the many forms different from it which the disease assumes; although, if we judged his knowledge by the above sentence, our judgment could not be other than adverse. In some parts we think the treatment proposed by our author quite as open to objection as his arrangement of his lectures and his terms. We take an instance:—

"In erysipelas of the face, incisions should not be made even though the tension be great, and even punctures are objectionable, on account of the deformity they would produce."

We would ask Mr. Spence whether incisions will produce as much deformity as extensive sloughing of the skin?

The lectures cannot be said to treat of diseases in any definite order, and the varieties of disease treated of in each lecture are not classified according to any general system. The author rarely notices in describing disease either the names of authors whose writings contain the best descriptions of the diseases of which he treats, nor does he give them any credit for their labours.

The volume has been well brought out; it is printed well and correctly, and the lithographs which illustrate it are (many of them) admirably executed. The coloured illustrations are perhaps inferior to the plain, but they are good representations of disease. One of the plain lithographs, that representing the appearances of the face of a patient suffering from tetanus, is most graphic.

RECENT WORKS ON DISEASES OF THE EYE.

(*Continued from p.* 390.)

The section on Cataract is replete with information on every possible detail. After a lengthened and learned disquisition on the anatomy, senile changes, nosology, symptoms, complications, causes, course, and results, we arrive at its treatment—to us the most attractive portion of the chapter. By treatment, Stellwag says:—

"We attempt to cause incipient cataracts to recede, or at least impede their course. Where the cataractous cloudiness is solely the expression of a recent inflammation, which is running its course, we may often fulfill this indication by proper antiphlogistic treatment; otherwise we must attempt to diminish, as much as possible, the disturbance of vision accompanying the formation of the cataract, until its removal from the axis of vision can be most easily and safely accomplished.

"*A.* 1. It is at least doubtful if cataracts can be caused to recede by therapeutic measures.

"It is true, several creditable authors say they have seen existing cataractous opacities clear up under the systematic use of mercury, and after frictions of iodide of potassium ointment about the eyes for months, after hydropathic treatment at Karlsbad, Eger, &c. (*Himly, Arlt.*) These cases, however, are such rare exceptions, that they scarcely encourage the commencement of such a plan of treatment. The hopes founded on the beneficial effect of the Karlsbad waters in the treatment of diabetic cataract (*Melchoir*) have been destroyed. The methodically-repeated paracentesis of the cornea is also shown to be inefficacious. (*Rivaud, Landray, and others.*) Still, some claim to have temporarily arrested the progress of cataract by this procedure. (*Secondi.*) The extent to which the use of concentrated sunlight may be serviceable in clearing up the cataractous opacities (*H. Langenbeck*) has not yet been satisfactorily decided.

"Medical treatment may, however, be of service in so far as it is suitable to remove direct or indirect causes of cataract.

"It can scarcely be denied that, with the removal of the cause, the development of the cataract may be easily impeded, and its progress restricted. But if this succeeds, it is evidently possible that the already cloudy portion may be caused to disappear by regressive metamorphosis and absorption, and a relative cure thus brought about. The indications for such treatment are clearest, when certain diseases exercise an injurious influence on the nutritive conditions of the whole body, and originate a pathological involution, as well as where local inflammations endanger the normal nutrition of the lens.

"2. But if the causes of cataract escape recognition, or if they lie beyond the reach of therapeutic means in unripe cataracts, it is best patiently to await their ripening, and direct our whole care to keeping off injurious influences, which may hasten the growth of the cataract, or place the other parts of the eye in a condition to endanger or prevent success in future operations."

We are sometimes consulted by patients having cataract in one eye with a view to its removal. The practice taught by Sir Wm. Wilde and other eminent and trustworthy practitioners is not to operate on such an eye so long as the second eye remains sound and useful; except in isolated instances of traumatic cataract or where the removal of the cataract is desired on grounds of personal appearance.

"If one eye is cataractous, while the functions of the other are normally performed or only slightly impaired, the operation should only be undertaken when the favourable results may be regarded as nearly certain; of course, this does not include cases in which the long retention of the cataract in the eye is itself injurious, as in traumatic cataracts which swell greatly, prolapsed lenses and parts of lens, &c.

"The advantages accruing to the patient, in case the operation is successful, are indeed not slight. First, we have the cosmetic interest, and in young persons this is often important, so that it alone may indicate the operation, even if want of functional power in the parts sensitive to light render restoration of vision out of the question. Besides this, both eyes are enabled to act together, the visual field is enlarged, the intensity of optical impressions and the correct judgment of small distances and dimensions increased. Another important point is, that, in the subsequent formation of cataract in the other eye, the patient is not at any time quite blind. (*Stellwag.*)"

It is still a moot question whether both eyes should be operated on for cataract at the same time. Stellwag remarks:—

"The question, whether both eyes shall be operated on at one sitting (where there is binocular, ripe, or over-ripe cataract), may be answered affirmatively in those cases where, after carefully weighing all circumstances, the result appears quite certain. But in proportion as the guarantees for the success of the operation diminish, the considerations against it increase. The most skilful and experienced ophthalmic surgeons contradict each other on this point, and contend with reasons that can not be refuted, but only weigh more or less heavily. (*Ritterich.*)"

Wilde and v. Graefe oppose the double operation. Macnamara says:—

"Supposing that in one eye the lens is opaque, and in the other tolerably transparent, should we wait till both eyes are equally involved before operating, or should we remove the cataract at once? It seems better to get rid of the opaque lens under these circumstances, because the retina is likely to deteriorate if not used; moreover, there can be no valid reason for keeping our patient in suspense and discomfort till he is completely blind, when by operating on the one eye he may be able to use it, while the degeneration is progressing in the other, and may thus be enabled to perform the ordinary duties of life without interruption.

"It may be laid down as a general rule, that, when both eyes are involved, only one should be operated on at a time; I hardly know of any circumstances that would make me perform a double extraction at one sitting, unless in the instance of double traumatic cataract, when we should do well to relieve both eyes as soon as possible from the irritation induced by the swollen and opaque lenses."

We differ from the author of the above when he says that the retina is likely to deteriorate if not used; in persons advanced in life, and affected with cataract, we have practical and conclusive evidence to the contrary. With children however the case is different; as Wells truly remarks:—"In them the passive suppression of the retinal image produced by the cataract, appears to exert a similar influence upon the sensibility of the retina, as the active suppression which occurs in cases of squint, and which often rapidly leads to great amblyopia." This is one of the principal reasons for advocating removal of the cataract during infancy. Of late years it has been proposed to accelerate the "ripening" of

cataract by opening the capsule, and thus hasten the period of operating. Stellwag says however that "forced ripening must be regarded as a very doubtful undertaking," and assigns his reasons. Wells seems to have abandoned it as he finds in v. Graefe's new operation a safe method of removing even unripe cataracts. Macnamara prefers removing an unripe cataract in its capsule. Removal of the cataract having been determined upon, the next question which arises is how it should be removed, and this is one of the great questions of the day. The easy but dangerous operation of couching or reclination has now but one advocate of any note—Quaglino of Pavia—and we are glad to find it condemned by our three authors. Stellwag says:—

"The chief danger, however, lies in chronic choroidal inflammations, which are excited by the dislocated lens acting as a foreign body. These occur after weeks, months, or even years, and after severe and tedious suffering they destroy the eye, and may even sympathetically affect the other one. The percentage of bad results from this accident is quite large; hence, at present, depression is almost abandoned."

Wells mentions the operation only "to state that in my opinion it should be completely abandoned," and Macnamara:—

"The native Buckeens and Kobrages always operate for the cure of cataract in this way, and hardly a week passes that I do not not see several of their patients at the Calcutta hospital, suffering from either inflammation of the choroid, or from retino-choroiditis. Here, at any rate, we have warning sufficient to prevent our resorting to this proceeding without considerable hesitation."

(To be Continued.)

The Surgical Treatment of the Diseases of Infancy and Childhood. By T. Holmes, M.A., Cantab.; late Surgeon to the Hospital for Sick Children; Surgeon and Lecturer on Surgrey to St. George's Hospital; Surgeon-in-Chief to the Metropolitan Police; Memb. Corresp. de la Soc. de Chirurgie de Paris. Second Edition. London: Longmans, Green, Reader, and Dyer. 1869.

This work is divided into three parts, which treat respectively of Malformations, Injuries, and Diseases. The arrangement is not strictly kept to, inasmuch as congenital hernia and hydrocele and orthopedics are, for convenience sake, brought under the third head.

The work, as a whole, gives evidence of research and a fair acquaintance with the writings of others, and is, moreover, very practical in its tendencies. It would be impossible to give an analysis of a book which deals with a subject of such extent; we shall, therefore, content ourselves with selecting some portions from each of the main divisions, and letting them tell their own tale. We shall premise only that, while we do not agree with all Mr. Holmes' views or conclusions, the pros and cons on debated points are fairly stated for the most part. We must, however, except from this commendation one expression which occurs in p. 473, line 13, in which, writing of a distinguished surgeon who has had singular good fortune in a limited number of cases, he uses the phrase:—"Even the most fatal operations may *by accident* be performed five times together with success." We have italicised the objectionable words, and hope that in future editions such an offence *contra bonos mores* will not disfigure the otherwise fair pages of Mr. Holmes. We agree with him in his estimate of the operation, and we do not agree with the gentleman in question; but we are also able to assure Mr. Holmes that much of the unwonted success of that surgeon was due to the extreme care and personal supervision which he bestowed upon the after-treatment of these cases.

In the first part, Ch. III., the subject of Nævus is fully dwelt upon. After a few words on such palliative treatment as pressure and cold, he observes:—

"The essential objection to the palliative treatment can hardly be better put than in the above words of Mr. Forster. After a tedious and annoying period of treatment, the surgeon sees no more of the patient, who is left with no possible security against the immediate relapse of the disease."

Nothing is said of the application of collodion or iodine, which will be found very efficient in minor cases, either separated or together.

What may be termed the radical treatment of nævus consists of ablation by the knife—an operation of early date restored to notice by Mr. Teale, jun., of Leeds, whose interesting communication on this subject was read before the Medico-Chirurgical Society, Feb. 26, 1867. Mr. Teale in this paper not only restored to notice this old and sometimes formidable operation, but suggested the preservation of the nævoid skin. The operation is one that may

occasionally be justified, but the preservation of the skin is a matter of more frequent possibility. There are many cases which can be cured by a temporary ligature sufficient to cut off some of the circulation, especially in the veins, but not sufficient to cause sloughing. Mr. Cooper Forster has suggested and practised this method with success, and it deserves more attention from surgeons than it seems to have met. It ought to be the rule where ligature is resorted to. Mr. Holmes mentions it merely in a casual paragraph. It is an old and favourite method with some surgeons in Dublin, and has been advocated in our pages.[a]

Mr. Holmes does not approve of subcutaneous ligature, because it sets up ulcerative inflammation and surgical fever, and is sometimes followed by the death of the patient. We shall let him speak for himself on the subject of caustics:—

"The use of the actual or potential cautery in nævus is very successful. I have repeatedly treated small nævi in exposed situations by passing a needle heated to a white heat in the blowpipe flame, into their tissue at various points, and I think this is a good way of treating those which are too thick to be dealt with by the use of the potential cautery:[b] for the latter penetrates only a very short distance at each application. But for nævi which are very superficial, no treatment, I think, answers so well as the repeated application of strong nitric acid. It should be applied carefully to the whole surface of the nævus, but not to the healthy skin around it, which may be defended with a layer of oil. The application of the acid substitutes a yellowish-white colour for the red of the nævus, and when the cure is complete it leaves a very slight depression of nearly the natural colour. It may require several applications, but rarely fails to cure nævi which are merely cutaneous. The application of potassa fusa will answer the same purpose, and as it penetrates deeper than the nitric acid, it is applicable to the treatment of thicker nævi; but it is much more painful, and leaves a more evident scar. These two substances answer their purpose so well, that I have not found it necessary to use any other of the potential cauteries.

[a] Dub. Quar. Jour., Vol. xliii., p. 21-2.

[b] A very convenient and manageable instrument for this purpose has been introduced into practice by Mr. Wordsworth. It is a curved pointed metal stem mounted on a wooden handle, something like the old-fashioned tenaculum. A short distance from the point the stem swells out into a bulb. The bulb and point are heated to a white heat in the blowpipe flame, and the point passed into the tissue of the nævus here and there. The heat of the bulb preserves that of the point, and obviates the rapid cooling which is the chief drawback to the use of the needle as described in the text. The "gas-cautery" invented by Mr. Bruce of the Westminster Hospital is also often convenient in the treatment of nævi.

"The electric-wire cautery is a very efficient and a very manageable form of the actual cautery, since it can be passed under a certain part of the tumour, and be gradually drawn out, dividing some of the vessels, and substituting a cicatrix in their place. Thus a portion of the tumour will be checked in its growth, and then another can be attacked. The plan is tedious, and the surgeon must be prepared for the possibility of hemorrhage occurring during the separation of the sloughs, but I have seen it successfully applied in the treatment of very large vascular tumours.

"Injection of the liquor ferri perchloridi is an efficient method of treating large nævi, and one which has the recommendation, when successful, of leaving hardly any visible trace of its action. But then it is not free from very serious dangers. It acts by coagulating the blood in the vessels, and thus obliterating them. Now if a large quantity of the liquid were thus thrown into a cavity of a vein of considerable size, and the blood in the vessel suddenly solidified, the most dangerous and even fatal symptoms might ensue. Cases of instant death under this treatment have been known; no doubt from the use of too large a quantity of the substance, and from the accident of its having been thrown into a large vein and there producing coagulation of a considerable quantity of blood, which has been carried direct in that state to the heart. . . .

"A case is also on record in which sudden death took place in injecting a nævus of the face with liq. ammoniæ."

In these remarks we fully concur. The perchloride injections are too dangerous to play tricks with. We have seen some narrow escapes with them.

There are few cases of nævus in which the temporary ligature, superficial or subcutaneous, nitric acid, hot needles, or setons, will not affect a cure, with time and patience. The great rule is not to go too fast, and to remember and allow for the contractile properties of cicatricial tissue. Surgeons and parents alike are apt to look for rapid and immediate results; and unless the slower processes of nature be taken into account, too much may sometimes be done by not allowing the great healing element of time to bear its part in our work.

The chapters on hare-lip and cleft-palate are full of important and interesting matter, and will repay perusal. The question of operating on young children under chloroform for these congenital malformations seems to have occurred to more than one surgeon. The question of priority in these matters is of no consequence, except so far as it shows that we on this side of the water are not asleep as regards improvements of practice, and that it behoves our

brethren on the other side to look sharp to their laurels, and to be cautious of crowning themselves prematurely. The actual priority in this case belongs to Dublin surgery. There is a capital case given of web-fingers quoted from a paper by Mr. Barwell in the *Medical Press and Circular*, where the deficiency of skin was supplemented from the haunch with success. It is remarkable in this case that the connexions were cut off on the fourth day, and but very little of the transplanted skin died.

We must now pass on to Part II. In the chapter on Wounds we find the following sensible remarks:—

"That even large and greatly lacerated wounds will often heal with extreme readiness in childhood, is no more than what the analogy of similar injuries in the adult would lead us to expect; for in healthy adults who are moderately free from irritability wounds will sometimes heal with extreme rapidity, and more especially in vascular parts of the body, and where the subcutaneous tissue is abundant. Now, in childhood, although in those who require operations there is often a tendency to scrofula, there is rarely that chronic visceral degeneration which is the most formidable obstacle to success in surgical operations on adults. Nor does the scrofulous taint materially interfere with the healthy union of wounds, as anyone might satisfy himself by watching a case of amputation for scrofulous disease. Children, again, though in one respect the most irritable of the human race, are in some others the most free from irritability. Their irritability is chiefly directed against sudden and acute pain; but confinement to bed and protracted disease, which wear out the patience and exhaust the hopes of older persons, soon become customary in childhood, and then produce little impression. Thus, children may be confined rigidly to bed, even sometimes in unusual positions, as on the prone couch, for months together, without suffering in any respect in their spirits or general health; whilst even the drain of a constantly suppurating wound produces much less impression than on older people. It is easy to see the main causes of these differences. I believe one of the chief, if not the greatest of all, is to be found in the freedom from mental depression in childhood. Another is, that the viscera which effect the depuration and regeneration of the blood are generally free from the disorders produced by the wear and tear of life, or by dissipation; and the third is, that the whole vital force is adapted to a period of growth, and is therefore in excess of what is required merely for the repair of the daily consumption. These conditions, favourable for the repair of wounds, are in part balanced by others unfavourable, as far as the immediate result of injury is concerned, viz., the great effect which sudden shock has upon the nervous, and

hemorrhage has upon the circulatory, system. Such unfavourable effects, however, are usually transitory; and hence I have heard it said by operators that a healthy child, if it can be got off the operating-table alive, will probably recover. This assumes that the secondary complications of wounds are rare at this age; and so they undoubtedly are, at least in English hospitals. I have lost a few patients at the Children's Hospital from pyæmia; only one, as far as I can remember, from diphtheria, and none from erysipelas or phagedæna. In one case (of excision of the hip) the whole wound became gangrenous, and the child sank rapidly; in another (of lithotomy) he sank, and died on the following day, without any obvious cause."

Further on is a note which in these days of feverish novelty is positively refreshing:—

"NOTE.—I would not wish to be understood as being an opponent of acupressure—in fact I often use it, and in some cases I think it a very great improvement—but it is the privilege of impartial observers to see objections which the enthusiasm of inventors and partizans causes them to overlook. In this matter of small oozing vessels I have found much embarrassment occasionally in using acupressure. If each vessel is to be secured, the mass of needles and pins with which the wound is filled is very awkward, and causes difficulty in bringing the flaps together, as well as in arranging the needles so as not to interlock with each other (see a case reported by Dr. Gillespie in the *Edinb. Med. and Surg. Journal*, June, 1865). If, on the other hand, the oozing is disregarded, the flaps may become distended with blood-clot; as happened to me the other day in amputating a breast with acupressure, when from this cause the union was far more tardy than it would have been with ligature. On the other hand, two or three extra ligatures take up hardly any room; and, in spite of all we have been told about the danger of 'fresh points of sloughing,' 'setons of dead matter bound into the wound,' &c., they exercise, I am bold to say, no deleterious influence whatever on the healing of the wound."

We must pass over the chapters on Fractures and Dislocations, which contain much interesting matter. Chapter XVI., on Burns and Scalds, contains the following:—

"The main objects of the surgeon are: 1. to relieve or obviate irritation; 2. to prevent destruction of tissue as far as possible; 3. to avoid or to remedy distortion.

"1. The first object is best effected by dressing the part as rarely as is consistent with cleanliness, and, if the pain is great, under chloroform, and by keeping the patient moderately under the influence of opium. I have never seen any harm from the judicious use of opium in early life.

"As to the materials of dressing, I attach less importance to the precise substance used than to the care which ought always to be taken that it shall form an unirritating case or crust tolerably impervious, and at the same time easily removed when it may become necessary to do so. The ordinary plan at St. George's Hospital (and it seems to me one of

the best in practice) is to cover the burnt surface with ceratum calaminæ spread on thin linen, and to defend the parts from the air by a thick coating of cotton-wool. Instead of the calamine cerate, any other bland unctuous substance may be substituted. We frequently use the Carron oil (a mixture of equal parts of linseed-oil and lime-water), and some surgeons prefer a mixture of collodion and castor-oil, as more impervious and equally cleanly. I have found in the few cases in which I have tried it that Mr. Lister's mixture of carbolic acid and linseed-oil (in the proportion of one of the former to four of the latter) with the paste made by adding common whitening to this mixture, answers well in burns. If the surface is small and the burn not deep, so that there is a prospect that the dressing may be left adherent to the end of the cure, the surface may be dusted with flour or covered with whitening or plaster, which may be kept on with a bandage until it cracks away. In superficial burns or scorches, any cooling and slightly astringent fluid will afford relief. I know of none better than common writing ink.

"2. In order to prevent destruction of tissue as far as possible, the greatest care ought to be used in removing the burnt clothes without disturbing the parts, and in preserving the epidermis as far as possible. The whole surface should then be carefully dressed, and the dressing left until it becomes absolutely necessary to change it. The warmth of the part is to be maintained by enveloping the whole in a thick layer of cotton-wool.

"The patient's powers must be supported by stimulants, given in such doses as can be borne without excitement. When suppuration begins, all portions of slough should be removed as soon as can be done without violence, and some local stimulant applied. Picked oakum is a very good dressing in this period."

To obviate contraction he insists on the careful and persistent use of instruments, and for its cure he has little to add to our previous experience of the futility of operation in most cases. One point, no doubt, is well brought out—namely, that failure in some cases is certainly due to want of perseverance in mechanically opposing the contraction of the cicatrix by proper instruments.

A valuable addition to our operations for remedying the effects of these cicatrices in the case of the lip, is to be found in the late Mr. Teale's new mode of building up a barrier to the overflow of saliva:—

"The treatment of contracted cicatrices may be said generally to be conducted upon one or other of the following principles, viz. 1. To place the scar between two constant but gentle forces of extension and counter-extension. 2. To divide the scar, or the skin near the scar, put the part

in its natural position, and maintain it so by suitable apparatus till the wound has healed. 3. To divide or to excise the scar, restore the parts to their natural position, and then fill up the wound so made with a flap of skin.

"All these plans have been recommended and practised, with every variety of modification, on cicatrices of the neck; and to these the late Mr. Teale of Leeds has added another measure, which we will first discuss, inasmuch as it cannot properly be said to deal with the scar itself, but is directed mainly to remedy one of its chief inconveniences—the unnatural position of the lower lip, and consequent sialorrhœa. Mr. Teale's operation was as follows. When the lower lip is drawn downwards, so that the teeth are exposed, its central part is first pared of the red upper edge, and is separated from the lateral portions by a vertical incision on either side. The lateral portions (which still retain their mucous edge) are then loosened by an incision running along the base of the lower jaw, and the flaps so formed are made to meet in the middle line over the central portion, which has been previously pared, their lower edges being united to the upper raw edge, made by paring the central part. Thus, the lower lip is, as it were, built up and the saliva retained. If the teeth are displaced so as to project against the lip, they must be previously extracted. In other respects, Mr. Teale's method of dealing with these cases does not differ from those of other surgeons. The operation on the lip above described is either used by itself or added to the usual methods of dealing with cicatrices."

We had marked other passages in Part II., but want of space compels us to pass on to Part III., on Diseases.

Writing of struma he quotes approvingly Sir W. Jenner's subdivisions of the diathesis into tuberculosis and scrofulosis. The former is the typical disease in the bright, fair, lively, sanguine child, the latter in the dull, phlegmatic, thick-skinned child. The distinction is good and useful, ever remembering that in practice rigid lines do not separate diseases absolutely from one another.

In cancrum oris he prefers nitric acid and the hot iron as a caustic—it is more efficient and searching, less alarming to patient or friends (for the patient should be chloroformed), and it does not produce bleeding as the cautery iron will do when, beginning to cool it sticks to the soft parts and tears them. He has no confidence in the chlorate of potash, although he continues to use it, apparently in deference to a harmless prejudice in its favour. In our opinion the chlorate is of use in combination with the muriated tincture of iron, which it unquestionably renders digestible with

ease and rapidity. In all cases iron and stimulants are the sheet-anchors. Strange to say, Mr. Holmes does not mention iron.

We are disappointed that one who has such opportunities of seeing bone disease should have so little to say on the active treatment of acute and chronic ostitis and caries. These are just the affections where we think the judicious interference of the surgeon may many times cut short disease which would otherwise go on to destruction of limb, joint, or even life itself. A bolder surgery in the early stages of ostitis would often stop its spread. Gouging of carious bone is too often left undone or done when too late. In many cases of incipient hip disease, inflammation commences in the great trochanter, and can be checked before it destroys the joint by timely incisions into the substance of the bone itself. Mr. Holmes is somewhat misty on the subject of hip disease; he does not, as appears to us, clearly distinguish between the acute synovitis and the chronic ostitis and their results. The first is the origin of acute morbus coxarius, and is characterized by shortening (apparent) width and fulness of nates and great pain; it runs on quickly into abscess and destruction of all the joint structures. The other is slow and insidious, begins with a limp and pain in the knee, and goes on by slow stages (with occasional exacerbations) to ulceration of cartilage and ulcerative absorption of the joint and its surroundings. As a describer of the pathological changes in diseased bone, Barwell is far before Holmes. As the aims of the books were different, it might be considered that Mr. Holmes might overlook the pathology, but the result of his doing so is an indistinctness of teaching both on the theory and practice of the subject which detracts from the value of his book.

On the subject of excision of the hip he writes as follows:—

"Excision of the hip is an operation which I have practised, I believe, more extensively than any other surgeon, and on which therefore I may perhaps be permitted to speak with some claims to be heard. The number of cases of which I have preserved notes is, I find, nineteen, besides some of which I have no record; and I shall give presently the exact result of this long series of operations. But I would wish, before producing this statement, to say a few words with respect to the general question of judging of operations by their results. The results will always vary according to the kind of cases operated on. I daresay this seems a truism, but it is, at any rate, a fact which is very little dwelt upon in treating of the subject. I mean this: if a surgeon restricts any operation, say that of excision of the hip, to the best or most curable cases of con-

firmed disease, he will obtain a good percentage of successes; but the question will remain whether the same success might not have been obtained by the expectant method. If, on the other hand, he restricts himself to cases in which, according to all reasonable probability, spontaneous cure is impossible, and operates upon every case in which the patient is at all in a condition to allow of his surviving the operation, then his tale of successes will be much less; but then also all the successes must be reckoned as clear gain.

"Now, the latter is the course which I have adopted in excision of the hip. I have never, or hardly ever, proposed the operation in any case where there was any reasonable probability of natural recovery. I do not say that this is the right course to adopt, nor do I think so; but the operation was almost an untried one in my experience, and I was obliged to make myself practically acquainted with it as best I could. This fact, however, will account for many of the unsuccessful events in my series of cases; while others are accounted for by an accidental liability to pyæmia which showed itself in the Hospital for Sick Children some time ago, and which came and went without any ascertainable cause. This unfortunate event deprived me of some of the most promising of my cases of excision of the hip, and still further reduced the number of my successful cases.

"If we wish to look accurately at the results of excision of the hip, we shall not be content with the common rough classification into death and recovery. Many of the deaths are unconnected with the operation; and in many cases, though the patient recovers, the operation fails.

"I think a fair classification for practical purposes might be made by separating the Deaths into those who die from the direct results of the operation, and those who sink from constitutional causes; and the Recoveries into those in whom the wound entirely heals and the limb is perfectly useful; those in whom the limb is useful, but the wound remains open for an indefinite period; and those in whom the patient recovers, but with a more or less useless limb and open wound—in fact, relapses into much the same state as we usually find in chronic hip-disease.

"Taken in this way, my nineteen cases will show, in the first place, seven deaths, in six of which I should refer the fatal issue to the direct effects of the operation, five of them dying from pyæmia, and one of gangrene of the wound. The other died of causes that had been acting, I believe, before the operation, which had, in fact, been put off till the patient was in a dying condition.

"In one of these cases, which died of pyæmia, the consequence of acute osteomyelitis of the femur, I amputated the limb at the hip, with the desire, if possible, of removing the cause of the pyæmia, but unsuccessfully, inasmuch as deposit in the lungs had already occurred, as was shown by *post mortem* examination. In another case I amputated with

success, the operation having been followed by chronic osteomyelitis of the femur. Rapid recovery ensued; but the patient had had cerebral symptoms before the operation, and he died of abscess of the brain some months after amputation. Both these cases are published in the *St. George's Hospital Reports*, Vol. i.

"Two other cases have died since the operation, but at periods of time very remote from that of the excision, and from causes quite unconnected with it. In one of these cases (Isaac Richards) disease showed itself in the opposite hip to the one excised, and soon went on to abscess. The boy lingered for a long while in an asylum for incurables, where he ultimately died. In the other case (Margaret Horing) the child, though she recovered from the operation, never had any use of the limb, which remained in a chronic condition of suppuration.

"This leaves nine cases, one of which (Alfred Davis) is, I think, in an incurable condition, and will probably ultimately die. One who has been twice operated on (William Morgan) I have not seen for a long while. When last seen he was improving in general health and in flesh, but the limb was much shortened and distorted, and there were still open wounds leading to softened bone. In two others (Lydia Smith and George Punter) I think ultimate success is likely to be obtained, though in the former certainly with much deformity of the limb. In two other children (James Tapson and Lydia Bygrave) the sores are nearly healed, and the limb is very useful. Success is nearly certain, I should hope, in their cases.[a] Two others are walking about with useful limbs, the wounds being perfectly sound (William Watts and Mary Ann Hall). In Margaret Kirby's case the result was equally good; but the child died some time after recovery from an accidental attack of pneumonia (not tubercular), and I have therefore the opportunity of bringing forward the annexed illustrations, showing the condition of the joint after successful excision.

"Thus, out of 19 cases,

"6 died from the direct effects of the operation (in one case after amputation.

"1 died after the operation from the previous effects of the disease.

"1 died of independent disease sometime after recovery from amputation.

"2 recovered from the operation, but not from the disease, and died a long while afterwards.

"2 were little if at all benefited.

"1 (twice excised) was doubtful.

"3 have useful limbs, but with sinuses.

"3 recovered completely.

[a] Since writing this, I have seen the former of these two patients, who has perfectly recovered.

"This would be a very unsatisfactory account of any of the other excisions; but whether it is so with respect to that of the hip, I am doubtful."

On excision of the knee, as Mr. Holmes has been quoted aganist the operation, we think it right to give his own statement of opinion:—

"1. I do not wish here to spend time upon controversial topics, which have, I regret to say, at times led to almost personal feeling; but I should not be stating the case fairly, if I did not say, with reference to the first of the three points above mentioned, that I believe excision to be a more severe operation than amputation, more immediately dangerous to life, and requiring a longer time for convalescence. I have repeated this opinion in several different publications, and have, strangely enough, been represented on this account as an opponent of the operation; the fact being that I have never willingly lost an opportunity of performing it. But, however impressed we may be with the advantages of an operation, that ought not to blind us to its severity, otherwise we shall be in danger of giving a very fallacious prognosis to our patients, and of exposing ourselves to constant disappointment and self-reproach. Far, indeed, from being an opponent of the operation, I am so sincere an admirer of it, that I believe it able to bear the truth to be told about it. I sincerely believe that it has, as practised hitherto, been more fatal than amputation; and I can see no reason whatever why it should under any circumstances be less fatal than that operation, it being in my judgment a proceeding of at least equal if not of greater severity. But even if I thought that excision would always continue to prove more fatal than amputation, I should still practise it, because I think its results, when it succeeds, are so good, that we are justified in running some extra risk to secure them."

With regard to the removal of cartilages of incrustation in amputation at joints, he writes:—

"Note.— Some surgeons, amongst whom is Mr. Butcher, believe that there is danger in leaving the cartilage exposed in the wound of an amputation. The idea probably originated in consequence of the importance attributed to ulceration of the cartilages in diseases of the joints; but it is singular that it should so long have resisted the conclusive experience of the harmlessness of the exposure of cartilages in the amputations of the hip, shoulder, and fingers. It is, I believe, simply a delusion. In the excisions of the tarsus, particularly that of the astragalus, the wound is often composed in the greater part of its extent of exposed cartilages, including in that operation the cartilages of the ankle-joint, of the two joints of the os calcis and that of the scaphoid. Yet I have quoted and figured instances above which prove how healthily such wounds unite."

This is not the only place he is down upon Mr. Butcher; he does not approve of his saw, at least he sees no advantage in it. In the matter of the cartilages, however, we are satisfied he is right; their presence is not in the least prejudicial to primary union, and we look on their removal not only as unnecessary but as positively likely to cause the very mischief it has been supposed to prevent.

On prolapse of the rectum the following judicious remarks are worth extracting:—

"When all these means fail, the prolapsed mucous membrane may be painted with nitrate of silver or nitric acid; and further than this I have not as yet seen reason to go, though many very severe cases of prolapsus are brought to the Hospital for Sick Children; and I am quite sure that operations for prolapsus would be of the rarest possible occurrence in childhood if patient treatment in bed were methodically followed out for a few weeks in such cases as prove more than usually troublesome."

In Enuresis he relies much on the habit of wakening the child about two hours after going to sleep, with due regard to diet and bowels, and the administration of $\frac{1}{8}$ gr. belladonna three times a day. Trousseau's caution on this point is worth inserting here—namely, to give a sufficient dose of belladonna once a day only (*Clinical Medicine*, Vol. i., p. 675, New Sydenham Society's edition). We must take leave of Mr. Holmes with regret. We could have lingered much longer over his book. He has compressed an immense amount of sound surgery into comparatively small space, and we could readily have doubled the quotation from it with profit to our readers. We have found fault with him for a few things in no carping spirit, but because his book is a good one and we wish to add our little towards making it perfect. It is an honest book, the record of the practice of a man of thought and of experience in this department, and we cordially recommend it to our readers. It is abundantly illustrated; the illustrations are generally good—those on pages 313 and 617 being notable exceptions.

PART III.

QUARTERLY REPORTS.

REPORT ON SURGERY.[a]

By Edward H. Bennett. M.D., and C.M. T.C.D., F.R.C.S.I., M.R.I.A.; Surgeon to Sir P. Dun's Hospital; University Anatomist, and Lecturer on Clinical Surgery in the School of Physic.

Since the attention of surgeons was directed by Sir. J. Simpson to the advantages of acupressure as a means of arresting hemorrhage, many efforts have been made with the same object, and many substitutes have been suggested for acupressure. The object of all these efforts is to avoid the ulceration and suppuration caused by ligatures, and by obviating these unavoidable consequences of ligature to effect the primary union of wounds, and to escape the dangers of secondary hemorrhage and of pyemia. Mr. Bryant,[b] while he admits that acupressure has some advantages over the ligature, regrets that both his observation and experience of it are unsatisfactory; he leaves the final decision on its merits, as compared with those of the ligature, to be made by experience, and discusses those of torsion, which he believes deserving of being referred to the same ultimate test. Mr. Bryant accounts for the fact that torsion has not been practised, although known to surgeons since the publication of Amussat's paper in 1829, by supposing that it was suggested before its time—namely, before the use of chloroform, "when the surgeon's anxiety was to complete his operation as speedily as possible, and it was a source of satisfaction to know that a stout ligature was on the end of a large vessel."

Mr. Bryant prefaces his account of his investigations of the effects of torsion with the statement that there are two methods of applying torsion described by Amussat—namely, "free" and "limited":—

"In free torsion the end of the artery should be fixed by a pair of clasp forceps, and twisted freely.

[a] The author of this Report, anxious that every contribution to surgical literature should be noticed, will be glad to receive any publications on surgical subjects. If sent to correspondents of the Journal they will be forwarded.

[b] On the Torsion of Arteries as a means of arresting Hemorrhage. Medico-Chirurgical Transactions, Vol. 51.

"In limited torsion the artery, having been drawn out of its sheath, should be fixed transversely about three-quarters of an inch from its divided extremities by a pair of clasp forceps and held steadily; whilst, with a second pair, the free end of the vessel should be twisted freely as in the former, the object of the first pair of forceps being to fix a limit to the twisting of the vessel, and prevent the artery being separatedfor any distance from its vascular attachment."

The relative merits of these two methods are thus described:—

"Free torsion appears to be the more applicable to small vessels and even to the large trunks of the extremities; the limited torsion to such arteries as are unsupported by muscles and loosely connected."

The number of revolutions requisite to close the vessels are determined by the experiments performed to be three or four for small vessels, five or six for large; when the vessels are atheromatous the former number suffices under all circumstances. In performing the operation care should be taken in all cases, and the orifice of the vessel alone should be held by the forceps, which twists it either in free or limited torsion; and in no case should the end be twisted off, as this part is shown to be essential to the safe closure of the vessel. Want of attention to this latter point, the importance of which will be seen in the description of the resulting physiological effect which we quote from Mr. Bryant's paper, has, in his opinion, contributed much to the failure of the method of torsion; for where it has been practised the end of the vessel has generally been twisted off. Of this we have seen several instances ourselves, and the result has been that, after one or two pieces have been twisted off, a ligature has been applied to the vessel. Mr. Bryant says that it is a good rule in all cases to hold the end of the vessel for a second or two after the twists have been made, to allow the parts to be plugged with blood, and to allow the inner divided tunics to come in contact. The forceps should have broad flat ends; and on this point Mr. Bryant differs from Mr. Syme, who used a three-toothed forceps, the teeth of which pierced the walls of the vessel. In the case of large vessels the whole orifice of the vessel should be seized and firmly held. Such are the principal rules for the performance of torsion which Mr. Bryant thinks to be established by his experiments. His investigations were made on the dog and horse, and on the dead human body. He gives, also, the results of a *post mortem* examination of a

human brachial artery which had been secured by torsion thirty-five hours before the death of the patient.

In the experiments on the horse and dog the vessels were divided, and closed by torsion; and the animals were killed at periods varying from forty-eight hours to eleven days. The vessels were examined after being hardened in spirits:—

"The results of the experiments made upon animals, and the single observation I have had (says Mr. Bryant) an opportunity of making on the human subject, singularly accord; for in the cardiac and distal ends of the arteries which had been treated by torsion, I found the middle and internal coats of the vessels had been divided; that these coats had retracted in the direction opposed to the blood-stream, approximated, and overlapped; that at the cardiac end, in several instances, the coats had become partially incurved, as shown in the preparations and drawings of the carotid of the horse and the brachial of the human subject; and I found, also, that every vessel contained clot from the giving off of the first branch to the point of contact of the two inner divided coats; that lymph or organizable blastema was poured out around the divided extremities of the coats, and between them and the external cellular coat; also that a second smaller clot often existed between the distal end of the two divided inner tunics and that portion of the cellular coat which had been twisted.

"I found, moreover, that the twist which the cellular coat had sustained was clearly permanent, and that it did not untwist (this can be shown on the dead body). That in no single instance was there any evidence to indicate that the portion of vessel which had been twisted had lost, or was likely to lose, its vitality, or that the parts about the vessel had received any material injury."

In these experiments—

"Free torsion was practised in every case in the horse; yet the twist in the vessel was local, not extending high up the artery."

Mr. Bryant's experiment on the carotid of the horse shows that the method may be applied "to large vessels immediately above the giving off of a large branch" successfully, and that it may likewise succeed "when applied to diseased vessels in old subjects."

The mode of closure of a vessel by torsion seems, by Mr. Bryant's observations, to be in many points identical to that effected by the ligature, the details of which were so fully demonstrated by Jones in his work on Hemorrhage, published in 1810.

Mr. Bryant points out what are perhaps the only differences between the results of the two processes—that the division of the internal and middle coats is more complete where torsion is used, and that incurvation of the inner tunics occurred as a rule after torsion, while it "seems unknown after ligature." It would appear, therefore, from these observations that torsion possesses all the advantages of the ligature, or even more than it in the freer division, retractation, and incurvation of the inner coats, while the ligature affords the advantages of facility in application and of allaying the surgeon's anxiety with the knowledge that "a stout ligature" secures the end of the vessel. Against the use of the ligature there are to be urged the consequences which its presence and separation entail, which Mr. Bryant has in no case met with in his experiments with torsion, and which he sees no reason to fear. These necessary consequences are thus described by Jones:—

"A certain degree of ulceration and formation of pus appears to be the necessary effect of the ligature, and this ulceration takes place as near as possible to a newly-cicatrized part; and as it is a well-known fact that such parts very readily ulcerate, it is obvious that every possible means should be employed to prevent the extension of this ulcerative process."

Mr. Lister[a] hopes to remedy the injurious consequences of the ligature by treating it, and the parts involved by it, so as to ensure absence of putrefaction, and by adopting a ligature composed of animal tissue to provide for its removal by the action of the absorbents. As proposed in 1814 by Dr. Physick, he proposes to cut the ligature short and allow it to be retained in the wound; his proposition applies chiefly to the ligature of arteries in continuity; and as he concludes with the following words, "now that torsion has almost superseded the ligature in ordinary wounds," we infer that he is satisfied with torsion as an efficient means of closing divided vessels. Possibly, if torsion succeeds in establishing itself in general practice, it will be applied to the securing of arteries for the treatment of aneurism, torsion of the extremities of the vessel being practised after the division of it, instead of the application of a single ligature to the undivided vessel.

In Mr. Lister's first observations on the practice of cutting the ligature short and treating both it and the wound antiseptically

[a] Observations on the Ligature of Arteries on the Antiseptic System. *Lancet*, 3rd April, 1869.

with carbolic acid, a silk ligature was employed; in the last cat-gut was substituted for silk, as more likely to undergo absorption.

His experience of the antiseptic treatment of wounds in general having led him to believe that dead tissue is not necessarily thrown off by suppuration, unless altered by putrefaction, or artificially imbued by stimulating salts, Mr. Lister inferred that the part of the outer coat of an artery included in a ligature, and consequently deprived of vitality, and the ligature itself, if of a suitable kind, might, by the careful use of carbolic acid, be rendered inert as a cause of suppuration, and that they might finally become encysted or be removed by absorption.

His first trial was made on the carotid artery of the horse, and the wound healed without suppuration; six weeks after, the vessel was found to be occluded, and the silk ligature was found as yet unaltered, lying dry in a bed of firm tissue. The second trial was made on the external iliac vessel of the human subject, the operation being performed for the relief of a femoral aneurism. In this case "the wound became superficial without suppuration;" four weeks after the operation, the superficial sore being completely cicatrized—

"The patient was allowed to walk about her room; and just six weeks from the date of the application of the ligature she went down three flights of stairs, and walked for some time in the streets, and then up again to her room without over-fatigue."

After an interval of ten months of fair health she died by the rupture of an aortic aneurism. The *post mortem* examination showed that—

"At the narrowest part the artery was reduced to mere fibrous tissue, constituting a dense white band five-eighths of an inch long, from the middle of which was seen projecting, at one side, a round buff-coloured appendage about a line in diameter, somewhat obscured by a trifling amount of inflammatory condensation of texture in the immediate vicinity. On scratching this little body with the point of a knife, I (Mr. Lister) found it to be a very thin-walled capsule, containing the knot of the ligature, with two tapering ends, which were shorter than the thread was cut at the operation, while the noose had vanished altogether. The surface of the knot also showed clear indications of having been subjected to an eroding agency, similar no doubt to that exerted by granulations upon dead bone absorbed by them. Besides the remnant of

the ligature, the tiny capsule contained a minute quantity of yellowish semi-fluid material, looking to the naked eye like very thick pus."

In this semi-fluid material were found pus-cells, forming, however, but a small proportion of its constituents, rounded corpuscles of smaller size, fibro-plastic corpuscles, imperfect fibres, and granular matter; and lastly, *debris* of the silk ligature. Mr. Lister is surprised that the parts in immediate contact with the silk should have assumed so imperfect a structure—a structure which he says could not be called pus, although it was a very near approach to it. He attributes this change, occurring at so long a time after the application of the ligature, to the disintegration of the silk fibres, which he considers are unirritating in their smooth continuous condition, while the reverse is the case when they are jagged by partial absorption.

Dissatisfied with the results following the application of the silk ligature in this case, Mr. Lister looks to "animal ligatures" as a substitute; these, although he admits that they have been long since tried and abandoned as unsatisfactory, he hopes to make obedient to the antiseptic system, the more so as experience of that system has in his practice been such as the following sentence indicates:—

"After the experience which the antiseptic system has afforded of the disappearance without suppuration of large dead pieces of skin and other textures, there can be little doubt that threads of animal tissue, if applied antiseptically, would be similarly disposed of."

The next and last experiment made was with cat-gut ligatures carbolized; with these the carotid of a calf was tied, and within a month the parts were examined.

"On exposing the artery (says Mr. Lister) I was first much disappointed to see the ligatures still there, to all appearance as large as ever. But had I borne in mind what I had observed in some of my earlier cases of compound fracture, I should have been prepared to find these threads present in appearance, though absent in reality.

"Hence it might have been anticipated that the ligatures of peritoneum and catgut placed on the calf's carotid would, after the expiration of a month, be transformed into bands of living tissue. Such was, in truth, the case, as was apparent on closer examination."

The ligatures which had been applied to the distal part of

the vessel were gone *in toto*, nothing being left but a band of fleshy tissue connected with the artery, and retaining some of the inorganic substances of the cat-gut in its substance. Of the cardiac ligature the knot and part of the noose alone remained. Mr. Lister refers to a case of compound fracture, which he thinks should have prepared him for this rapid absorption of foreign animal matter, which he has recorded as follows:—

"A piece of lint soaked with undiluted carbolic acid had been placed over the wound, and formed with the blood a firm crust. Nearly three weeks after the accident I was detaching a portion of the adherent crust from the surface of the vascular structure into which the extravasated blood beneath had been converted by the process of organization, when I exposed a little spherical cavity about as big as a pea, containing brown serum, forming a sort of pocket in the living tissues, which, when scraped with the edge of the knife, bled even at the very margin of the cavity. This appearance showed that the deeper parts of the crust had been converted into living tissue; for cavities formed during the process of aggregation, like those with clear liquid contents in a Gruyère cheese, occur in the grumous mass which results from the action of carbolic acid upon blood; and that which I had exposed had evidently been one of these, though its walls were now alive and vascular. Thus the dead, but nutritious mass, had served as a mould for the formation of new tissue, the growing elements of which had replaced the materials absorbed, so as to constitute a living solid of the same form."

Had Mr. Lister's evidence of the absorption of the cat-gut ligatures been of the same kind as that which he here gives of the absorption of a crust composed of blood, lint, and carbolic acid, and of its replacement by living solids of the same form, we should not have devoted time or space to the examination of it. He gives, however, a detailed account of the microscopic appearances, which seems to contain proof of the reality of the rapid absorption of the cat-gut ligature. Mr. Lister shows himself to be sanguine as to the results to follow from his experiment, as the following passage indicates:—

"The surgeon therefore may now tie an arterial trunk in its continuity, close to a large branch secure alike against secondary hemorrhage and deep-seated suppuration—provided always that he has so studied the principles of the antiseptic system, and so carefully considered the details of the mode of dressing best adapted to the particular

case in hand that he can feel certain of avoiding putrefaction in the wound. For my own part I should now, without hesitation, undertake ligature of the innominate, believing that it would prove a very safe procedure."

This seems to be a large amount of confidence to be inspired by an experiment on the carotid of a calf.

A short article on the employment of carbolic acid in surgery, published in the *Bulletin General de Thérapeutique Medicale et Chirurgicale*, shows that authorities on the continent do not regard it with quite as much favour as some British surgeons do. Since, says our French contemporary with much justice, carbolic acid has become the fashion, it has been used, or rather abused, by attributing to it impossible properties; it obtains these marvellous properties, above all, in the hands of English surgeons, who are infatuated with it. Our French authority holds that in spite of the great powers attributed to it by Mr. Lister, the death-rate has been greater in Mr. Lister's hospital since its employment than it was some years ago before it was known; he maintains that the cases recorded of excision of the wrist, of fracture opening the ankle-joint, &c., cured without suppuration, are but exceptions such as every surgical method can claim; that such is the case to some extent seems clear from the following opinion of the merits of carbolic acid published by Dr. J. Bell:[a]—

"The cases I have reported are merely examples out of the many we have had, illustrating the same great practical fact, that the use of carbolic acid dressing as an antiseptic method, followed out with due care, and with the great precautions which are essential to success, is able in some cases practically to get rid of suppuration; in nearly every case greatly to diminish its quantity, and entirely to destroy its fetor."

It is probable that of the many cases in which it is used hardly any but those in which it succeeds in producing union without suppuration are published, while every surgeon who tries the treatment free of prejudice sees many good results from its use no doubt, but a vast number of cases also in which it seems to effect but little, if any, benefit. Our own experience would rather incline us to conclude that while it has in some cases apparently prevented suppuration, it has in these cases delayed the healing of the wound, which has remained in an inactive condition, and that

[a] Edinburgh Medical Journal, May, 1869.

on a change to ordinary treatment suppuration has occurred, and a rapid closure of the wound followed.

While we are slow to admit the innumerable powers attributed to carbolic acid, its antiperiodic, antisyphilitic, stimulant, anesthetic, nutritive properties, and many others, we fully admit its antiseptic properties, and believe, with the French authority we have quoted, that the presumption is that a very careful analysis of the facts would reduce them all simply to the results of this action. Dr. J. B. Russell[a] draws attention to the use of the vapour of carbolic acid as a disinfectant in hospital wards, and describes a convenient mode of using it in practice. His plan is to place a porcelain cup full of water with a smaller cup filled with pure carbolic acid supported in it over a gas jet or stove in the ward, and so to volatilize the acid and water into the atmosphere of the ward. He resorted to this as preferable to the vapour of chlorine or sulphur, and as being more under control, and hence more capable of being continuously used.

Our own experience of the effects of the vapour coincides with that of Dr. Russell—first, in the fact that it did no injury to the human beings in the wards in which it was used, and what we regard a matter of great importance, especially in surgical wards, it banished the flies so numerous in a hot summer. Dr. Russell says:—

"During the hot summer of 1868 the flies, which were so numerous and so annoying in wards not so fumigated, were only occasionally seen in wards furnished with the evaporating apparatus."

We have seen the effect produced during the same hot summer by the simple use of a dropping bottle filled with a dilute aqueous solution of the acid, and allowed to drop over the bandages of a recently-amputated stump in the ordinary way.

Dr. Lente describes a new mode of securing vessels in ordinary operations as follows:—

"Cut off a length of silver wire, say three inches—less will do, however, for small arteries—give it a sharp bend in the middle, then one or two twists as near the bend as possible. This forms the ligature. Mode of application:—Secure the twisted end of the ligature in a pair of spring or catch forceps, and it is ready. Draw out the vessel to be ligated, or, if a small one, the tissue in which it is, with a tenaculum or

[a] Glasgow Medical Journal, February, 1869.

[b] A new method of securing divided vessels, and on the use of Carbolic Acid, &c., by F. D. Lente, M.D. American Journal of Medical Sciences, April, 1869.

forceps in the usual manner; let an assistant apply the ligature against the vessel at the point where the two legs diverge from the twist; then the surgeon secures the hold by two or three half-turns."

"The surgeon, having twisted the wire, cuts off the end with ordinary scissors, within two or three turns of the artery; the other end, if not too long, may be left, as we thus avoid having two sharp ends to the ligature."

He directs that the wire should not be so thin or be drawn so tightly as to cut the arterial coats.

Dr. Lente claims to be the first who has applied the wire ligature for arresting hemorrhage in general operations; he mentions, however, the experiment of Mr. Broadhurst, in which the femoral artery was occluded for aneurism with a wire ligature, the wire being cut short and the wound closed. He holds that Dr. Howard has demonstrated by his experiments on animals, detailed to the New York Pathological Society, "that the compression of an artery by a ligature of whatever kind should not be sufficient to injure any of them (the arterial coats), especially in cases where disease of the arteries is suspected." He informs us also that Dr. Buck, of New York, has discovered a method of ligature of vessels for aneurism, which has been tried in several cases by him and his friends, but he states that he is not at liberty to anticipate Dr. Buck's publication of the method or its results. Dr. Lente tells us also that it is becoming "generally admitted by surgeons that it is better to compress the inner coats of an artery merely for the arrest of the current through it, than to destroy their integrity." We are informed that to Dr. Warren Stone, of New Orleans, is due the merit of having enunciated this theory and of having put it into practice as long ago as 1859. Surely Scarpa, Crampton, and many others held this doctrine and applied it in their practice. We are not prepared to admit Dr. Lente's statement that it is becoming generally admitted by surgeons that it is better to compress the coats of arteries without dividing the middle and internal, in order to secure their closure, for the great majority of surgeons still use the ordinary ligature, and use it so as to divide the inner coats of the vessel; and thus by their practice endorse the opinions of Jones as distinguished from those of Scarpa and Crampton. Such, to quote an American authority, are the directions which Professor Gross gives for the application of the ligature to large vessels, and that this is the practice of American surgeons generally no one can doubt who reads the

reports of the surgery of the late American war. The great advocate of acupressure, Sir J. Y. Simpson admits, that no sufficiently extensive series of experiments upon the lower animals, or of observations on the human subject, has yet been collected together to show the whole pathological mechanism by which arteries are closed when acupressure is used; and until this knowledge has been acquired we cannot expect to be able finally to decide on its merits, nor can we adopt it with the same confidence as the ligature, of which the effects have been so fully examined, and the failings and advantages of which are so well known.

The trials of acupressure reported in this Journal in November, 1867, by Dr. Porter, are of much interest, as they demonstrate that in some cases acupressure may be applied to large arteries for a considerable time without occluding them. Sir J. Simpson states that the largest arteries have been generally found closed within fifty hours; but we find, in the application of it to the axillary artery in continuity by Dr. Porter, that the vessel was not occluded after fifty-three hours; on the contrary, when examined more than six weeks afterwards, it was "not completely occluded, but narrowed" at the point where the acupressure had been applied. The mode of application of acupressure adopted in this case, the third method of Simpson—not the fourth, as stated in the report—was such as to completely close the vessel while it was applied; yet the sides failed to adhere to each other. The attempt to occlude the artery by the distal operation not having been successful (though it should be stated that no bad results followed its adoption), the acupressure was applied by Dr. L'Estrange's instrument to the innominate artery, and again failed to procure occlusion of the vessel, although maintained for fifty-nine hours and a-half, and, on part of the vessel at least, with such force as to induce sloughing of its coats.

In the February number of this Journal Dr. Porter figured and described an artery compressor, which he hopes will prove an efficient means of controlling the circulation; and there can be no doubt that it will effect its object more perfectly than either a probe and wire applied, as in the third method of Simpson, or than the instrument of L'Estrange. Of its accomplishing the ulterior object of acupressure, namely, the permanent occlusion of the vessel, we cannot judge except by experience; it is highly probable that it will in some cases succeed quite as well as the

instrument of Crampton, to which it is superior in being lighter, and capable of being applied with less disturbance of the connexions of the vessel. Its efficiency as a means of commanding the circulation in a large vessel seems to be well established by the fact that it has been tried with success in this respect on the human aorta in the case reported by Dr. W. Stokes, jun., in the present number of this Journal.

Dr. Watson communicated to the Medico-Chirurgical Society of Edinburgh, on March 2nd of this year, a paper on the treatment of aneurism by compression, in which he recorded two cases of its successful application; he brought these cases before the society, as he believed that they were "the first cases in which this method of treatment had been successfully employed in Edinburgh," and because in the treatment of them a modification in the method of compression by weights was devised, which he believes can be more easily borne by the patient than any other, which can as efficiently control the circulation in the vessel. Dr. Watson stated that in two cases pressure had been tried by the late Professor Miller without success, and that some years ago a military surgeon, educated in Dublin, had successfully treated a popliteal aneurism at Piershill; these, with a case in which he himself made trial of compression some years ago without success, are, he believes, all the cases in which pressure has been tried in Edinburgh. Before attempting to account for this fact, Dr. Watson says:—

"It may, perhaps, seem strange beyond belief that, in a surgical school such as that of this city, the first cases of the successful treatment of aneurism by compression should occur fully twenty-six years after this method of practice became an established procedure in Dublin, and after it has become generally accepted by the whole civilized world."

Two causes have, he holds, led to this strange rejection of the treatment by compression:—

"Firstly, the comparative rarity of cases of popliteal aneurism, since the introduction of railways through the kingdom has done away with posting and postboys. Secondly, the determined opposition, on purely speculative grounds, afforded to the employment of compression by those whose practical opportunities were greatest."

Dr. Watson does not leave us in any doubt as to the author of this opposition, for he quotes, in a note on the above passage, the decision published by Mr. Syme in 1847 against compression:—

"So long as it is my sincere persuasion that ligature of the artery is preferable to pressure for the cure of popliteal aneurism, I shall deem it my duty to pursue this method, though it may not perhaps be the best suited for the lowest capacity of surgical skill. Let every man act according to his powers; but let no one who feels it necessary to choose inferior means throw blame upon those who are able to practice a higher exercise of their art."

From his experience of the cases which he reports, Dr. Watson believes that to treat an aneurism successfully by compression in as short a period as possible, the pressure applied to the main trunk must suffice entirely to interrupt the circulation through that vessel, and that this interruption to the normal circulation through the sac should be as continuous as possible. Dr. Watson's instrument is thus described:—

"Carte's compressor for the thigh, with the metal arch heightened, the pad removed, and the elastic element withdrawn. There still remains the rod, no longer, however, acting as a screw, but sliding through the ball and socket joint beneath, so as to admit of the easy introduction of the weight. The weight is ovoid in form, and has a channel through its central long axis large enough to let the rod play easily without catching. The extremity of the weight by which the pressure is applied has the form of the larger end of an egg cut out of metal $3\frac{1}{3}$ inches in diameter. The inclination given to the rod by the universal ball and socket joint determines exactly the amount of weight and the situation at which the pressure is to be applied."

Dr. D. Peruzzi,[a] in a letter addressed to Dr. Boinet, claims for Italian surgery the honour of having introduced ovariotomy into Europe in 1815; he admits, however, that but little attention has been paid to the operation in Italy in later times, until the publication, in 1865, by him of an Italian translation of Mr. Baker Brown's work on ovarian dropsy. The operation to which he directs attention was performed by Dr. Emiliani, of Faïence, with a successful issue, in 1815, but no account of it was published until that recorded by his son in the *Bulletin of Medical Science* of Bologne in 1843. No notice seems to have been taken of the operation or of the date of its performance, and hence the statement is current in all standard works, including that of Dr. Boinet, that ovariotomy was first performed in Europe by Lizars at Edinburgh in 1823. Dr. Peruzzi concludes his letter with an

[a] Aperçu historique sur l'origine, et les progrès de l'ovariotomie en Italie. Lettre à M. le Dr. A. A. Boinet par le Dr. D. Peruzzi. Chirurgien Premaire de Lugo.

analysis of the cases in which gastrotomy has been performed in Italy since the year 1859 for the removal of ovarian tumours, or of fibrous tumours of the uterus, and as an exploratory operation. His table contains the results of twelve cases of the first class, two of the second, and three of the last class. Ten of the twelve cases of ovariotomy terminated fatally, the most frequent cause of death being peritonitis, which occurred in five cases; two patients died of shock, one of pleuritis, associated with tubercle; and in two cases the cause of death was not recorded.

The two operations undertaken for the removal of fibrous tumours were fatal in twenty hours and forty-eight hours respectively—the first from causes unascertained, the second from shock. In the exploratory operations, all of which terminated without ill effects, resulting from the opening of the abdomen, the tumours were found to be solid tumours of the ovary in the first case, fibrous tumour of the uterus in the second, and osteo-sarcoma of the ilium in the third; in none of the three cases was the removal of the tumour attempted. Dr. Peruzzi prefaces his analytic table of these cases, the mortality in which is 60·5 per cent., with the statement that his object in publishing it is that it may prove an assistance in completing the contemporary history of ovariotomy, however unsuccessful the results it records may be.

Mr. T. Spencer Wells laid before the Royal Medical and Chirurgical Society, during the present session, the statistics of three hundred cases of ovariotomy in which the operation was completed; the general mortality of these cases was 28·33 per cent. —a result which is widely different from that of the Italian cases of the same kind, of which the mortality is 83·33 per cent. No doubt results deduced from so small a number of cases as Dr. Peruzzi records can never be relied on; but an examination of the death-rate of each successive hundred of Mr. Wells' cases shows that we may expect to find in the next Italian report an improvement in the results of ovariotomy; of his first hundred cases he lost 34, of his second 28, and of his third but 23. Mr. Wells has been led by his experience to the following conclusions regarding the effects of tapping on subsequent success of ovariotomy:—

"1st. That one or many tappings do not considerably increase the mortality of ovariotomy. 2nd. That tapping may often be a useful prelude to ovariotomy, either by giving time for the general health to improve, or by lessening the shock when the fluid is removed a few days

or hours before removing the more solid part of an ovarian tumour. And 3rd. That when the syphon trocar is used in such manner as to prevent escape of ovarian fluid into the peritoneal cavity, and of entrance of air into the cyst, the danger of tapping is very small."

Mr. Smith[a] commences his paper on the cure of cleft palate in children by quoting the observations of Fergusson and Pollock on the difficulty of performing the operation necessary for the closure of clefts of the soft or hard palates before the age of fifteen, in consequence of its tedious and painful nature, and in consequence of the inadmissibility of chloroform in its performance. Langenbeck has set down as the result of his experience that "the age of seven years will be a limit not to be over-stepped, as a rule, and that one must abstain from employing the operation in younger children." The operation of closing the soft palate is stated by Langenbeck to have been attempted in eleven cases in children between the fourth month and sixth year unsuccessfully, while Billroth succeeded in curing a cleft of both hard and soft palate in a single case operated on during infancy. Langenbeck admits that though the difficulties are inexpressibly great in the performance of the operation in young children, still they are not insuperable; he attributes the failure to the impairment of the nutrition of the child caused by the difficulty and pain of swallowing while sutures are present in the palate, and to the suppuration early induced by it, causing a solution of the union which may have occurred in the days immediately following the operation.

Mr. Smith holds that "the imperfections of deglutition, taste, and smell, and the unnatural thirst may probably be remedied at whatever period of life the cure of the deformity is effected;" but he maintains that the union of the palate by operation is often accomplished too late to do more than slightly improve the defective articulation, though it may cure the nasal resonance of voice.

"A résolute and intelligent adult patient (says Mr. Smith), after a successful operation, may acquire by his own efforts, or with the aid of an instructor, the power of distinct and normal articulation. But in some the difficulty of unlearning an almost life-long habit is well-nigh insuperable. It is probable that were the deformity of cleft palate cured in childhood, before or whilst the power of speech is being acquired, there would be but little difficulty in teaching a child to articulate

[a] Medico-Chirurgical Transactions, Vol. li.

distinctly. It is at least certain that the earlier the cure was effected the less difficulty would the patient have in learning to speak plainly."

In the appendix to his paper Mr. Smith records facts observed in three cases, which prove that the closure of a cleft of the soft palate alone in a young child tends to diminish the co-existing fissure of the hard palate; and he believes that before long he will be able to prove that, provided the union of the soft palate be effected in early life, the operation on the hard palate will in many cases be unnecessary, and in some unadvisable."

In ten months after the closure of a cleft of the soft palate, the gap in the hard measured nearly four-tenths of an inch less than it did ten days after the operation; in the next case a considerable gap was reduced in fourteen months to the size of No. 6 catheter; in one month a third gap was reduced by quarter of an inch. It would appear, then, that in addition to the advantages of the early operation, in an educational point of view, its adoption promises to diminish the severity of the proceeding by getting rid of the steps required to close the hard palate. The success of the operation, in Mr. Smith's opinion, depends largely on the employment of a gag capable of maintaining the jaws open and fixed, while it dilates the mouth and holds down the tongue. Such an instrument he has devised, and the results of his operations performed with its assistance prove its efficiency. He recommends the use of chloroform in all cases. His table contains eleven cases, in all of which the gag was used and chloroform administered. Of these cases three only failed; the successful cases included children of the ages of two years and eleven months, three, five, six, nine, ten, and thirteen years.

Any one comparing the statements of Langenbeck, Fergusson, and Pollock, which have been quoted above, with the statement of Mr. Smith's success, will, we think, admit that he has made real progress in this difficult branch of surgery.

Dr. W. R. Whitehead[a] records a successful operation for cleft of the hard and soft palate performed on a child seven years of age, in which a gag effecting the same objects as that devised by Mr. Smith was used, and in which anesthesia was maintained by ether. In this case the entire fissure was closed in a single operation, the incisions being those recommended by Langenbeck. The operation was performed on December 16th, and Dr. Whitehead, writing in

[a] New York Journal, April, 1869.

the April number of the New York Journal, thus describes the result, so far as they relate to the probability of the production of bone in the flaps composed of the entire of the soft tissues covering the hard palate:—"The parts at present feel as if bone will ultimately be formed. This, however, I prefer to test a little later with a needle." The case must therefore be classed for the present at least, as one of failure in this respect; and probably it will remain so, for a sufficient time would appear to have elapsed for the production of some bone since the performance of the operation. The speech in this case, "which, before the operation, was unintelligible, afterwards became distinct and intelligible, but the nasal tone still persists."

Dr. Whitehead publishes an analysis of fifty-five operations for cleft palate collected from various sources; but we can place but little reliance on its results when we find in it such proof of inaccuracy and prejudice as the following passage contains:—

"In a single case operated on in France by Sédillot, on a child three years of age, there is nothing said about the speech; but the only object of this operation seems to have been to disprove that the periosteum alone can in this locality or elsewhere reproduce bone."

In proof of Dr. Whitehead's inaccuracy we may state that Dr. Sédillot's patient was thirteen years of age, not three, and the condition of speech is described as being "nasonnée et inintelligible" before the closure of the cleft palate; and after it had been closed its condition is thus described:—

"L'enfant parle beaucoup plus distinctement, et se fait aisément comprendre."

No one can read the account of the performance of the operation given by Sédillot, and his conclusions drawn from the facts of the case, and believe that his only object in operating was, as stated by Dr. Whitehead, to disprove "that the periosteum alone can in this locality or elsewhere reproduce bone." True, he doubts the power of the periosteum to produce bone in the flaps, but he does not absolutely deny it; for he says:—

"Si nous ne les avons pas observées de notre côté, nous n'en contestons nullement la possibitié, et nous nous bornons à réclamer des preuves positives, et incontestables, pour posséder la conviction scientifique d'un fait aussi important et aussi fécond en conséquences ultérieures."

But of the results of the operation as performed by Langenbeck, which he calls "cette belle operation," he says that a multitude of sufferers, condemned up to the present day to bear their deformity or have recourse to obturator plates, will demand the assistance of surgery as soon as they shall know its resources and happy results. He sets himself to prove also that though there be no production of bone following the operation, at least there is no danger to the existing bone to be feared from the stripping of its periosteum essential to the performance of the operation.

REPORT ON MATERIA MEDICA AND THERAPEUTICS.[a]

By Walter G. Smith, M.B., Dub. F.K., and Q.C.P.I.; Senior Demonstrator of Anatomy in the School of Physic; Assistant Physician to the Adelaide Hospital.

MATERIA MEDICA AND GENERAL THERAPEUTICS.

1. *Syrup of Iodide of Iron.*—M. Jeannel has observed that the addition of one five-thousandth part of tartaric acid, renders syrup of iodide of iron clear when it has decomposed, and at the same time notably diminishes its inky taste.—*Bull. de la Soc. de Pharm. de Bordeaux.*

2. *Granular Citrate of Magnesia.*—This so-called compound really consists of soda and magnesia, combined with sulphuric, tartaric, and carbonic acids, and containing sugar with some citric acid, flavoured with essence of lemon.—*Pharm. Jour.*, Oct., 1868.

3. *Chanteaud's Soluble Oxide of Iron* (*Sucre Ferrugineux.*)—This compound occurs in small cubical reddish-brown crystals, with a purely sweet taste, followed by a slight chalybeate flavour. Being desirous to ascertain if the iron were really combined with the sugar in a soluble form as asserted, I made a few experiments on a sample procured from M. Chanteaud's agent in London, with the

[a] The Author of this Report, anxious that no contribution to the subjects of Materia Medica and Therapeutics should remain unnoticed, will be glad to receive any publication which treat of them. If sent to the correspondents of the Journal (see page iv.) they will be forwarded.

following results:—*Not a trace* of the iron is really in solution, but the peroxide is simply adherent to the surface of the crystals of sugar, for when it is digested in cold water and *carefully* filtered, the filtrate when acidified gives negative evidence with the tests for a ferric salt. The amount of iron present is extremely small, and as the result of two experiments I obtained 0·47% and 0·48% of peroxide of iron, respectively. Hence, to give a dose of this soluble peroxide of iron or saccharated oxide of iron, equivalent to 5 grs. of the ferri perox. hydr., the minimum dose in the British Pharmacopœia, we should have to order nearly 2½ oz. (1,041 grs.) For further details see *Pharm. Jour.*, July, 1869.

4. *Spontaneous Changes in the Strength of Solutions prepared for Subcutaneous Injection.*—M. Bourdon, of Paris, having met with several cases in which subcutaneous injection failed to produce the anticipated effects, was led to believe that this was due to a spontaneous loss of strength in such solutions as had been kept for several months. From his observations it appears that solutions of vegetable alkaloids are decomposed, and, therefore, lose in strength by the production in the first place of vegetable organisms which feed as it were on the dissolved alkaloid, and in the second place after these organisms have attained a certain development, by the deposition on them of an additional portion of the alkaloid salts.—*Amer. Jour. of Med. Sci.*, April, 1869, from *Bull. Gén. de Thér.*, April, 1868.

5. *Progress of the Cultivation of Cinchona.*—Besides the localities in India where cinchona plantations have flourished for some years past, nurseries of Peruvian bark trees have been established with success in Jamaica by Mr. Wilson and Mr. Thomson, and at the close of 1867, there were 25,000 healthy plants of the different species of cinchona in that island. In the Mauritius and the Azores, also, the results of cinchona cultivation are encouraging, and young plants have been raised with success in Chili, while for the last four years, cinchona condaminea has been growing in the Experimental Garden of Agriculture at Washington.—*Repert de Pharm.*, Mars. 1869.

6. *Counter-irritation.*—We may accept as one advantage at least of the lively interest at present taken in therapeutics, that it brings prominently forward the creeds of men of different modes of

thinking. Within a short time, five papers[a] have appeared on the doctrine of counter-irritation and its application to practice, and the importance of the question at issue demands a brief account of the views of their respective authors.

The first on the list is Dr. Dickinson, and the whole burden of his able though sceptical paper is to show that counter-irritation is useless, and indeed ridiculous, except when there is a direct vascular or nervous connexion between the part of skin irritated and the morbid part. After passing in review all the paths by which the several parts of the body communicate with each other, viz.:—the blood vessels, the absorbents, the nerves, and contiguity or apposition of structure, Dr. Dickinson comes to the conclusion that "none are adapted to convey the traditional value of counter-irritation in the direction which it is reputed to follow," and finally urges that "we may, therefore, cease to apply irritations to the skin of the head in disturbance of the brain; to the back in affections of the spinal cord; to the chest in diseases of the lung, and in general forbear to apply remedies to the parts which have no direct vascular connexion with the structure diseased, unless the remedies are of such a kind and such a magnitude as to bring the whole system under their influence." Dr. Dickinson is of opinion, and truly so, that "therapeutical traditions long survive the theories which gave them birth," yet it is quite possible, as Dr. Risdon Bennett has observed, that these traditions may sometimes survive, because they possess some inherent principle of vitality, independent of the theories in which they are supposed to have originated, and it may fairly be asked, "are we always to assume that the new theories are so unquestionably placed so far beyond the region of doubt, that all therapeutical traditions must be at once and for ever abandoned, if found to be in opposition to the theories of the day?"

Dr. Dickinson finds it difficult to believe that "excoriations of the surface of the body, in themselves trifling, can produce deep-

[a] On Counter-irritation considered in reference to the remote and indirect effects of local morbid changes. By W. H. Dickinson, M.D.; Assist. Phys. to St. George's Hosp. St. George's Hosp. Rep., Vol. iii., 1868.

On the Popular Idea of Counter-irritation, by F. E. Anstie, M.D., F.R.C.P. Lancet, Feb. 27th, 1869.

On Counter-irritation; a Theory constructed by the Deductive Method of Investigation. By James Ross, M.D. 1869.

On Counter-irritation. By R. B. Painter, M.D., F.R.C.S. Lancet, April 24th, 1869.

Remarks on Counter-irritation. By J. Risdon Bennett, M.D., F.R.C.P. Practitioner, June, 1869.

seated alterations in unconnected though neighbouring organs," nor can he understand how "bedaubing the chest with tincture of iodine can modify the course of tubercular disease in the apex beneath," or that a superficial vesication can promote the restoration of a hepatized lung, and by a not very happy analogy, compares such reasoning to that which would ascribe a formation the result of a profound geological change to an artificial modification in the surface of the adjacent soil.

While we welcome gladly every advance in the *science* of medicine, we cannot and need not always look for a corresponding advance in the *art* of medicine, and it can scarcely be prudent to reject dogmatically the experience of ages, even though it be not in exact accordance with the physiology and pathology which are the boast of this century. The maxim—"*Sufficit scire quid efficiat etiamsi modus nos lateat*"—is still applicable to many of our most potent remedies, and if these levelling views were pushed to their full and legitimate extent, respecting other methods of treatment, our therapeia would soon be limited within narrow bounds. The following remarks of Dr. Bennett are apposite to this point:—"Medicine as an art, notwithstanding the rapid advances it is making as a science, is still, and certainly for a long time to come, will remain based to a large extent on observation—cultivated scientific observation if you will, but yet observation, as apart from theory. Practical medicine has a distinct domain of its own, into which physiology has not yet proved its right to intrude. It has its own evidence, which can neither be ignored nor refuted, however much science may seem to dispute its validity."

Dr. Anstie supports and mainly concurs in Dr. Dickinson's arguments, but is not so sweeping in his denunciations of counter-irritation, and while he sees something "eminently ludicrous" in the idea of relieving pneumonia, or inflammatory enteric affections, by blisters in their vicinity, he instances particularly the value of judicious blistering in the treatment of neuralgia as an example of the transmission of a beneficial action through the channel of the nerves, and he admits that in some cases of inflammation the use of the milder irritants (or stimulants, as he prefers to call them,) just sufficiently near to the inflamed part, instead of increasing the tendency to dilatation of the vessels, adds something to their tone, and assists them in maintaining that healthy balance which is opposed to the spread of the inflammatory process. Indeed, both Dr. Anstie's and Dr. Dickinson's papers contain facts and statements

which themselves furnish strong evidence for the practice they decry.

In a pamphlet recently published, Dr. James Ross very ingeniously endeavours to establish logically the theory that the real nature of the influence exerted by counter-irritants is that of a stimulant, and that the influence transmitted from an inflamed surface to a neighbouring morbid texture is conveyed through the medium of the parenchyma, *i.e.*, from cell to cell, not only when the structures are continuous, but also when they are merely contiguous. We are reminded that a very extensive series of changes may be set in motion within the body by a cause which would be regarded as inappreciable if measured by any known standard of comparison, except the effects which it produces on the living body. Dr. Ross attempts to show that the various empirical laws or practical maxims which direct the practice of counter-irritation are in harmony with the conditions of his theory, as tested by the laws which ought to govern a scientific hypothesis. Though it cannot be said that Dr. Ross has unequivocally demonstrated his theory, or that his arguments are altogether free from objections, he deserves our thanks for his thoughtful attempt to work out a therapeutical hypothesis by the received principles of logical investigation.

A plain and rational plea in defence of counter-irritation has been put forward by Dr. Painter, and he illustrates by a number of facts how an impression on one part of the body may influence beneficially a distant part, although there may be no *direct* vascular or nervous connexion, and without positively affirming how, in a given case, counter-irritation may be of service, points out several ways in which great good may be effected by such means. He draws especial attention to the possibility of the continuity of the morbid nutrition being interrupted by the impression or new action set up by the irritant, just as a bad mental habit may be broken by a sudden necessity or shock.

But the practical utility of counter-irritation has quite recently found an able and energetic advocate in Dr. Risdon Bennett, and he adduces such a variety of instances, drawn both from nature's own operations and from therapeutic experience, many of which must be familiar to every "practical physician," as should convince the most sceptical of the value of counter-irritation despite our inability to give a plausible explanation of its mode of action in each case.

Defining counter-irritants as "agents which, by their irritant

action, determine an increased attraction or flow of blood to one part of the body, and thus influence morbid action in some other part," he thereby excludes the consideration of such means as setons and issues as having a much more complex action. The following examples of the proper use of counter-irritants will probably be admitted by all careful clinical observers:—

We know that the sudden exposure of the chest of a delicate susceptible person to a draught of cold air is frequently followed by pectoral symptoms, such as a sense of tightness and soreness of the chest, with more or less cough, in other words, by evidence of disturbance in the functions of the bronchial mucous membrane; and, on the other hand, daily experience teaches us that the distressing sense of tightness and dyspnea, and harassing cough, attendant on a common cold, are strikingly benefited by the application of a mustard plaster to the chest. There can be no doubt that sudden modifications in the capillary circulation of the exterior of the chest are capable of inducing correlative changes in the pulmonary circulation. A blister will often afford manifest relief in a paroxysm of cardiac asthma with attendant pulmonary congestion, and the paroxysmal exacerbations of dyspnea, occurring in the latter stage of phthisis, may often be mitigated more distinctly and speedily by counter-irritation to the chest than by any other remedy. External irritants to the chest have a decided influence in relieving the pulmonary congestion that is apt to supervene in continued fever, provided the depression of vital power be not so great that the capillary action of the skin remains uninfluenced by the counter-irritant.

And, without multiplying indefinitely therapeutical instances, are we not taught by many natural analogies that disease in one part of the body can be influenced by the appearance of disease in another and perhaps far distant part? We see bronchial affections in children relieved by the occurrence of an eczematous eruption on the scalp, and *vice versâ*, and the development of an asthmatic paroxysm in the adult is frequently seen to follow on the subsidence of some chronic cutaneous eruption. The natural alternation of gouty affections often gives a hint for the successful treatment of some of its protean manifestations, and there are many facts that may be urged in favour of the Hunterian doctrine as to the antagonism of disease.

Turning now to abdominal affections, Dr. Bennett vindicates the use of blisters in hepatitis, ilio-cecal inflammation, and ovarian

irritation; while blisters or other forms of counter-irritation will often distinctly relieve obstinate vomiting, especially if attended with pain and tenderness. With these clinical data, then, before us, "are we to refuse to believe that tissue changes and modifications of cell growth can be effected simply because there is no direct communication, whether vascular or nervous, between the seat of our counter-irritation and that of the disease? Who shall tell us what is the precise kind or amount of influence requisite in order to modify those molecular changes in which the local disease consists? What influence stimulation of distant nerves, whether vaso-motor or other, may have on the capillaries of the diseased part, either in the way of augmenting or retarding cell-growth, it may be difficult to say, but it can scarcely be affirmed that physiology is opposed to any such assumption."

Until physiology is able to unravel the sympathetic relations between certain organs, such as the well-known consensus between the breasts and the mammæ, or between the testicles and the parotid, or can explain why burns, especially burns of the abdominal integuments, should induce ulceration of the duodenum, it seems at least premature to reject or deride a practice which appears to have "not only authority, but also sound reason in its favour."

7. *Carbolic Acid.*—Dr. Fuller, in the course of many observations made in St. George's Hospital, finds that, so far as the mere dose is concerned, six or seven minims are a full dose, and that most persons, especially women, began to complain when the dose had been increased to eight or ten minims. The unpleasant symptoms complained of were a sense of burning in the throat on swallowing the acid, and giddiness or fulness in the head. He states that the acid imparts a greenish tint to the urine, and causes the disappearance of all deposits of lithates, but did not appear to exercise any special beneficial action in gout or rheumatism. It seemed to be chiefly useful in cases of fermentative dyspepsia, and has operated beneficially where charcoal failed to give relief.—*Brit. Med. Jour.*, Feb. 20th, 1869.

8. *The Methyl and Ethyl Derivatives of Atropia and Conia.*—Drs. Crum Brown and Fraser, following up their important and suggestive researches on the connexion between chemical constitution and physiological action, have lately examined the physiological action of the salts of the ammonium bases derived from atropia and conia. The atropia derivatives examined were the sulphate and

iodide of methyl-atropium, and the iodide of ethyl-atropium. These authors have found that the action of the methyl-and ethyl-derivatives is essentially the same on the sympathetic system, but on the cerebro-spinal nervous system it is different from that of the natural base. The chief effects produced by atropia on the central nervous system are excitation of the spinal cord and paralysis of the motor and sensory nerves, and it is now announced that the methyl-and ethyl-derivatives of atropia combine with the ordinary paralysing action of atropia an additional amount of paralysing action, bearing some ratio to the absent spinal stimulant action of the natural base, and consequently these derivatives affect the motor nerves much more powerfully than atropia itself. Paralysis of the vagi nerves and dilatation of the pupil are caused by these derivatives of atropia.

Conia, as usually prepared from the hemlock plant, is a variable mixture of conia (*i.e.*, *normal* conia) and methyl-conia. The substances examined by the authors were—1st, conia; 2nd, methyl-conia; and 3rd, salts of dimethyl-conium; and they find that the salts of conia and of methyl-conia very closely resemble each other in action and in poisonous activity.[a] The salts of dimethyl-conia differ from those of conia and of methyl-conia in never directly producing convulsant effects or other symptoms of abnormal activity of the reflex function, and in being much less active as poisons. They have demonstrated, in frogs, that the paralysis from all three substances is due to a curare-like action on the terminations of the motor-nerves.—*Proc. Roy. Soc. Edinb.*, Jan. 18th, 1869.

MM. Pélissard, Jolyet, and Cahours have lately confirmed Drs. Brown and Fraser's researches on conia and its derivatives in all essential particulars.—*Compt. Rend.*, 1869, p. 149.

9. *Santonin.*—In the *Bull. Gén. de Thér.* for April, 1868, some experiments of Professor Giovanni on santonin, and his conclusions as to the cause of the yellow colour, which objects present to the eyes of those who have taken it are summed up.

It is well known that santonin becomes yellow when exposed to the light, and it causes the urine, when exposed to the light, to assume a yellowish-green colour, and, according to Drs. Giovanni and Ambroise, often induces hematuria.

When santonin is taken, after having already turned yellow, the

[a] According to M. Landrin the poisonous activity of conia has been much overrated by Christison and Geiger—(Rev. de Ther., Oct., 1868.)

urine is *not* affected in colour, and objects do *not* assume the yellow tinge to the patient. The blood also, which, when exposed to the air, after *unaltered* santonin has been taken, becomes yellowish, undergoes no change in the former case. From this Professor Giovanni concludes that it is in the eye itself that the blood changes colour in consequence of exposure to light as it passes along, and that where any of the santonin penetrates into the aqueous humour it is in this medium that by its yellowish hue the yellow and greenish-yellow spots seen by the patient are produced. As there is some danger of causing serious derangement of vision by the permanence of this dye, the author advises that either santonin should never be given until it has already changed colour, or else that it should be given only at night, in which case the visual powers will not be endangered.—*Half-Yearly Compend. of Med. Sci.*, Philadelphia, July, 1868.

10. *Report on Mercury, Podophyllum, and Taraxacum.*—The report of the Edinburgh committee of the British Medical Association on the action of mercury, podophyllum, and taraxacum, on the biliary secretion, which was drawn up by Dr. Hughes Bennett, has just been published, and the conclusions arrived at are so directly opposed to the current belief in the cholagogue action of these drugs that extreme interest and importance must attach themselves to results which have been reached by such distinguished men as were on the committee.

It is right, however, to state that Dr. Christison, one of the committee, "cannot concur entirely in the conclusions drawn," and thinks that a force is given to the experimental facts which the facts themselves do not sustain. Drs. Maclagan, Fraser, and Gamgee, also do not altogether sanction the observations of Dr. Bennett, because the report was not submitted to them, as members of the committee, before publication.

The term cholagogue is used in the sense of a remedy which is supposed to stimulate the action of the liver, and so promote an increased flow of bile from that organ.

All the observations were made on dogs, and these animals in particular were selected as best suited for the purpose, because it was shown that the action of mercury on their salivary glands, mouth, intestines, appetite, and general nutrition, was the same as in man, and hence it is in the highest degree probable that its action on the hepatic secretion will also be the same. Moreover, the qualitative composition of canine is the same as that of human

bile, and the dog, like man, can be fed on a flesh, vegetable, or mixed diet.

The experiments and observations detailed in the accompanying tables seem "to show that pil. hydrarg, calomel, and corrosive sublimate, when given to dogs in either small, gradually augmented, or in large doses, do not increase the biliary secretion; they do not even influence it so long as neither purgation nor impairment of health are produced, but they diminish it as soon as they do either or both."

The results of observations made in reference to podophyllum and taraxacum are as follows:—

1st. Doses of podophyllin (resin of podophyllum) varying from 2 to 8 grains when given to dogs diminished the solid constituents of the bile, whether they produced purgation or not.

2nd. Doses which produced purgation lessened both the fluid and solid constituents.

3rd. During an attack of dysentery both the fluid and solid constituents of the bile were greatly lowered.

4th. Doses of the solid extract of taraxacum, varying from 60 to 240 grains affected neither the biliary secretion, the bowels, nor the general health. It appears, also, that purgation, produced by a variety of causes, either spontaneous, dysenteric, or from mercury or podophyllum, diminishes both the fluid and solid constituents of the bile.

11. *Comparative Toxicology.*—As instances of how differently vegetable poisons act on the lower animals and on man, and as illustrations of the caution required in conducting physiological inquiries or deducing therapeutical conclusions from experiments on inferior animals, the following facts are worthy of being borne in mind.

Dr. Weir Mitchell (*Amer. Jour. Med. Sci.*, January, 1869) has shown that pigeons are remarkably insusceptible to the toxic action of opium, though most poisons affect the pigeon readily enough, and, so far as he is aware, no other creature is thus protected from the action of opium. Amongst many other experiments he gave to a large pigeon which, within the two preceding days, had swallowed 42 drops of black drop, 21 *grains* of powdered opium within four hours. No serious consequences ensued, and the pigeon was well and active the next day.

Dr. B. Ward Richardson has since confirmed these statements by further experiments.—*Br. and For. Med. Ch. Rev.*, April, 1869.[a]

[a] In reference to Dr. Mitchell's experiments, Dr. Fraser, of Edinburgh, observes that such examples of insusceptibility may be divided into two great classes—1st, Those that are dependent on causes for which absolutely no explanation can be sug-

Dr. Elliott, of Hull, found that 1 oz. of shag tobacco, eaten by a goat, had no effect at all, and 10 minims of nicotia subcutaneously injected caused loss of power in the limbs, &c., but in twenty hours after the operation the goat was quite well.—*Br. Med. Jour.*, May 22nd, 1869.

Dr. S. R. Percy, in his interesting paper on atropia (*Richmond and Louisville Med. Jour.*, April, 1869, see No. 14) referring to the medico-legal bearings of belladonna as a poison, points out how important it is to decide whether the lower animals are affected similarly to man, and whether the flesh of such animals as may exhibit immunity from the action of large doses of deadly night-shade, or other vegetable poisons, is poisonous to man? Such a question did arise some time ago in England, in the case of a surgeon named Sprague, who was accused of placing atropia in a rabbit pie for the purpose of poisoning his wife.

About forty years ago Runge demonstrated that rabbits suffered no inconvenience when fed for eight days on the leaves of belladonna, stramonium, and hyoscyamus, though the alkaloid was absorbed, for the urine of the rabbit was found to exert a mydriatic action on the eye of a cat. These observations were confirmed by Reisinger, and have been amplified lately by Dr. Ogle. It is equally probable that hares share this immunity from danger. Dr. Percy asks—"Does this immunity exist with rabbits only? Is belladonna the only vegetable poison that may be eaten without injury? May not these animals have a like immunity with hyoscyamus, digitalis, aconite, lobelia, &c.?" and the following experiments afford a partial answer to these inquiries:—Belladonna, hyoscyamus, and digitalis, were administered to goats and kids in large quantities without producing any serious toxic effects, but the pupils were enormously dilated, and the urine of a kid, to which 6 drachms of extract of belladonna had been given, was thrown into the stomach of a young dog, and proved fatal to the animal. Atropia was detected in the urine of the poisoned animal by the physiological test, as well as by chemical and microscopical examination.

No evidence of atropia could be extracted from the heart of the dog, or from its voluntary muscles, *e.g.*, of the hind leg.

Two rats were killed by the subcutaneous injection of sulphate

gested by the knowledge we at present possess of normal and pathological physiology, among which may be placed the immunity of the serpent to its own venom; and 2nd, Those that are dependent on special varieties of organization in animals of different species, among which, probably, may be placed the insusceptibility of pigeons to opium.—*Jour. of Anat. and Phys.*, May, 1869.

of atropia (1 gr.–1½ gr.), and the carcase of each of them, minus the viscera, given to dogs, who ate it readily, and suffered no inconvenience whatever. Atropia was found in the urine of the rat, but none in any of the solid tissues of the body, which were examined as soon as death occurred, four hours after the ingestion of the poison. Though we cannot deny that the flesh of wild or domestic animals may be poisonous to man from some poisonous vegetable eaten by them, we see from Dr. Percy's experiments that where atropia was given to animals, and death caused by its action, the flesh of the poisoned animal was not poisonous, and failed to yield chemical evidence of having been poisoned.

12. *External Use of Digitalis as a Diuretic.*—Dr. Brown has succeeded in re-establishing the renal function in six cases of calculus of the kidney, when all other measures had been tried without effect, by the external use of digitalis in the form of poultices made either by throwing the fresh leaves into boiling water, or by incorporating the concentrated tincture with linseed meal. A rapid fall of the pulse follows the application. The cataplasms made with the leaves are especially to be recommended, and they should be renewed every six hours till the lowering of the pulse warns us to desist.—*Rev. de Thér.*, Dec., 1868.

13. *On some of the Accidents resulting from the Internal Use of Nitrate of Silver, and especially Argentine Albuminuria.*—From the observations made by MM. Charcot and Vulpian on man, it appears that there is a succession of physiological phenomena following the prolonged use of nitrate of silver. In the first period, the only appreciable phenomenon are itching, or a papular erythema. The second period is marked by a deep blue border on the neck of the teeth, first described by M. Duguet, and which usually comes on at the end of three months, when at least two grammes of the salt have been taken. Dark stains are visible over the greater part of the buccal cavity. The slate-blue discolouration of the skin, the *argyria* of the Germans, supervenes in the third period, and is not usually produced until a considerable time after the appearance of the gingival line. In addition to this characteristic phenomenon various observers have determined the presence of black granulations in most of the tissues and viscera, thus testifying to the general impregnation of the system.

M. Liouville has recently called special attention to a consecutive lesion of the kidneys, attended with albuminuria, observed in a woman, aged thirty-four, who had been treated for locomotor ataxy with nitrate of silver for nearly nine months. The kidneys, on

section, showed numerous distinct, bluish-black points, which were the Malpighian bodies stained by the metal. The tubes were not much altered in colour, but numerous spots of degeneration were seen, recalling the granular and fatty lesions of Bright's disease. Albumen had several times been detected in the urine of this patient during life. The pyramids were not so evidently impregnated, but the suprarenal capsules were manifestly so. This species of albuminuria just alluded to is comparable to the saturnine albuminuria described by M. Ollivier, and physicians will see the necessity of henceforth examining the state of the urine in patients who have been submitted for some time to the administration of nitrate of silver.—*Gaz. des Hop.*, 1868.

14. *Atropia.*—Dr. S. R. Percy has published in the April number of the *Richmond and Louisville Med. Jour.*, a valuable prize essay on the chemical and toxicological properties of atropia, and on its physiological and therapeutical action.

Some of his remarks bearing on its toxicology have been already given. From experiments made on dogs, Dr. Percy finds, that as a rule, if atropia is given without water, the effects are much more irritative, and last much longer than when a sufficiency of water is allowed. When death takes place, where water is not allowed, there is always congestion of the kidneys. Morphia, although an antidote to atropia in ordinary cases where water is freely allowed, is hardly an antidote where fluids are entirely withheld. A much larger dose of atropia may be borne without danger, if care is taken to keep the system well supplied with fluids, and the effects of poisoning pass off much more rapidly if warm diluents are prudently administered; but if large doses of the drug are given, unaccompanied with liquids, the kidneys are unable to eliminate either the poison or the urea, and the animal consequently dies either from uremic poisoning only, or from the conjoined toxic effects of the alkaloid and of uremia.

The physiological observations made on man agree with those of other inquirers. Passing by the therapeutic use of atropia as an anodyne, which, though so generally recognized in this country, is denied by some American authors, viz.:—Mitchell, Morehouse, and Keen, Dr. Percy speaks highly of its value in the treatment of passive constipation, in doses of $\frac{1}{30}$ gr. of the sulphate in a wine-glassful of water, particularly in delicate and nervous females, and states that he has in many instances given permanent relief by the careful use of sulphate of atropia alone, or if need be combined with tonics and aperients. The dosage should be managed so as to

produce the first degree of its physiological action, viz.:—dilated pupils, a dry throat and a little thirst, and Dr. Percy judiciously insists on the necessity of an effort being made to empty the bowels every day, after breakfast, so as to establish the habit of periodicity. Dr. Percy, like most other authorities, disbelieves in the vaunted prophylactic virtues of atropia against scarlatina, and also in the assertion that belladonna is liable to produce a cutaneous eruption, an assumed "fact" well worthy to be classed along with other Hahnemannic "provings."

In reference to the interesting question of the antidotal powers of opium and belladonna, the author thinks that there is, upon the whole, abundant evidence to prove that opium and belladonna, morphia and atropia, are mutually antidotal, and notices that the dilatation of the pupil from atropia can be more quickly overcome, if a solution of sulphate of morphia, 4 grs. to ʒi. is applied to the eye by means of the eye-cup. Two cases have lately been recorded confirming Dr. Percy's opinion. In the one case, M. Abeille used the subcutaneous injection of morphia with success, to counteract atropic poisoning in a child aged six years, who had swallowed by mistake 5 centigr. (0·77 gr.) of sulphate of atropia. (*Rep. de Pharm.*, Nov., 1868). Conversely Dr. M. S. Buttles relates a case of opium poisoning from the subcutaneous injection of gr. ss. of morphia, in which the hypodermic injection of $\frac{1}{60}$ gr. of sulphate of atropia, repeated within half an hour, proved successful after artificial respiration had been kept up for one and a half hours without avail.—(*Amer. Jour. Med. Sci.*, Jan., 1869.)

SPECIAL THERAPEUTICS.

15. *Nitrate of Lead in Onychia, &c.*—Dr. de Moerloose speaks most favourably of the good effects of applying powdered nitrate of lead to the sanious fungous ulcers resulting from onychia. A single application of this salt every twenty-four hours is sufficient, a light bandage being put on over the diseased surface, and it is stated that after the first dressing, the pain ceases, the swelling decreases, suppuration is lessened, and the fetid odour is destroyed. Of course, if any irregular horny fragments are seen at the bottom of the ulcer, it will be necessary first to excise them. The most rebellious cases are cured often in eight or ten days, at most in three or four weeks. An extension of its use might be made to certain obstinate scrofulous and unhealthy ulcers. (*Rev. de Thér. Med.*, Oct., 1868.) Dr. J. G. Wilson is convinced that the local application of nitrate of lead is superior to any other agent in the treatment of excoriated

or fissured nipples. The formula he generally employs is 10 gr. of nitrate of lead in ʒi. of glycerine applied freely to the affected nipple.—(*Glasg. Med. Jour.*, May, 1869.)

16. *Calabar Bean in Tetanus.*—In the *Glasgow Med. Jour.* for May, 1869, Dr. W. Fenwick reports the use of calabar bean in two cases of acute traumatic tetanus. The first case terminated fatally, but the remedy was not given until the disease was far advanced, and, by some mistake, was omitted during one night, though manifest good effects had been produced by it. In the second case, a child aged nine, was ordered 2 grs. of the powdered bean with $\frac{1}{3}$ gr opium every three hours, the symptoms of tetanus having been developed for some days previously. The opium was soon omitted, and 3 gr. of calabar bean with 3 gr. of rhubarb given every four hours during the day. The improvement following the use of the bean was most marked; thrice it was stopped, with the same result, increase or resumption of spasm; three times it was resumed with the same result, viz.:—cessation of the spasms. The patient altogether took about 300 grs. of the powdered bean, and recovered perfectly.

Dr. Greenleaf (*Richm. and Louisville Med. Jour.*, May, 1869), adds seven cases, collected from various sources, some under his own care, to the twelve already reported by Dr. Fraser of Edinburgh, illustrating the value of physostigma in the treatment of tetanus. (See *Edin. Med. Jour.*, May, 1869.) Three recovered and four died, so that with Dr. Fraser's cases, we have a total of nineteen cases of tetanus treated with calabar bean, twelve of which recovered and seven died. Of the seven additional cases, however, related by Dr. Greenleaf, in one case the *post mortem* examination revealed the presence of a hairpin imbedded in a wound in the scalp, a persistent source of irritation. If this be excluded, as well as a case of Dr. Watson's in which only one dose of the bean was given, we have a result of *seventeen cases* treated with only *five deaths*, a degree of success that has not been shown to follow the employment of any other remedy. Dr. Greenleaf considers the alcoholic extract of the bean to be the best preparation for use, either by the mouth or hypodermically, giving in the former case from $\frac{3}{4}$ gr. to 1 gr. every two hours, in the latter $\frac{1}{4}$ gr. to $\frac{1}{3}$ gr. increased as the effect passes off, or as the urgency of the symptoms demands.

17. *Oil of Turpentine, an Antidote to Poisoning by Phosphorus.*—M. Personne has performed some experiments on dogs which go to prove that oil of turpentine, if given immediately after the poison, or even in one or two hours, is capable of acting as a complete and

safe antidote. The explanation of its action which M. Personne offers is, that as phosphorus kills by interfering with the aeration of the blood, which it deprives of its oxygen, so the essence of turpentine when absorbed seems to prevent the oxidation of the phosphorus in the blood, in the same way as it hinders the combustion of phosphorus in the air. By depriving the phosphorus of the power of abstracting oxygen from the blood, it allows of the poison being eliminated without disordering the system.—(*Rep. de Pharm.*, Mars., 1869.)

18. *Atropia an Antidote to Prussic Acid.*—M. Preyer has demonstrated on rabbits and guinea-pigs, that the subcutaneous injection of a very small dose of sulphate of atropia, if performed pretty quickly after the taking of the prussic acid, is an unfailing antidote.—(*Practitioner*, Aug., 1868.)

19. *Application of the Hypodermic Method to the Treatment of Syphilis by Mercury.*—The advantages claimed by M. Bricheteau for this mode of treatment are—1st, Syphilitic symptoms rapidly disappear; 2nd, Certainty and precision of application; 3rd, Diminution of relapses; 4th, Facility of application. M. Lewin injects $\frac{1}{8}$–$\frac{1}{4}$ gr. of corrosive sublimate, but M. Bricheteau recommends a solution of double iodide of mercury and sodium as less likely to cause local irritation. (*Practitioner*, March, 1869.) Professor Sigmund has also made extensive investigations on the use of subcutaneous injections in syphilis. He uses a solution of 4 grs. of corrosive sublimate in oz. 1 of water, and usually injects 15 minims $= \frac{1}{8}$ gr. The smallest quantity of the salt by which a cure was effected was $1\frac{3}{4}$ grs.; the largest quantity required was $6\frac{1}{2}$ grs. (*Brit. Med. Jour.*, March 27th, 1869; quoted from *Wochenb. der K. K. Gesellsch. d. Aerzte*, No. 49, 1868.)

20. *Acute Orchitis.*—Mr. Furneaux Jordan states that he can cure acute orchitis in *twenty-four hours*, by at once painting the scrotum with a strong solution of nitrate of silver (℥ii. to ʒi.), while a stripe of vesication is excited over the adjoining femoral artery, *i.e.*, the next vascular territory by means of linimentum iodi.

Mr. Noble Smith has also employed a solution of caustic (℥i. to ʒi.), with the best results in the treatment of the out-patients at St. Mary's Hospital, who suffer from orchitis; using an evaporating lotion for the first few days.—*Br. Med. Jour.*, Jan. 1st, 1869, and Feb. 6th, 1869.)

21. *Treatment of Orchitis by Application of Constant Continuous Currents.*—Drs. Chébon et Moreau-Wolf recommend in the *Rev. de Thér.*, the employment of continuous currents in orchitis, and

relate nine cases, some gonorrheal, and some traumatic, in which galvanization was practised every day. After each sitting the pain was notably lessened, and the cure was complete in from six to thirty sittings.

The principal advantage gained by this method of treatment is that it does not interfere with the patient pursuing his ordinary avocations. The current from ten to twenty-four Remak couples, at most, was directed across the tumour for seven or eight minutes; immediately after, the positive pole was placed on the most painful point of the swelling, and the negative pole on the track of the cord for four to six minutes; thirdly, an *ascending* current was directed along the course of the cord.—(*Ann. et Bull. de la Soc. de. Gand.*, Mars., 1869).

22. *Progressive Muscular Atrophy.*—Dr. Wilks gives a case of a girl, aged twenty-four, in whom this disease existed in its most aggravated form, having steadily advanced for eighteen months. Regular faradization produced decided improvement in two months, and in six months she was discharged convalescent, and soon after regained a ruddy plump appearance.

Dr. Nasemann cured a very advanced case of paralysis of all the limbs with the continuous current. The cure was complete in less than eighteen months, and improvement took place after only one week. The current was derived from twenty Siemens-Halske cells; the positive pole was placed on the nape of the neck, and the negative pole on the cervical sympathetic, for five minutes on each side. The induced current had failed to do good.—(*Practit.*, Dec., 1868; quoted from *Berlin Klin. Wochenschr.* 37, 1868.)

23. *Infantile Paralysis.*—Dr. Russell Reynolds (*Lancet*, July 11th, 1868), thinks that the interrupted current is, on the whole, the most efficacious treatment. Sometimes the muscles will only respond to a continuous current. The treatment must be patiently employed often for many months. Dr. Hammond (*Quart. Jour. Psych.*, July, 1868), has treated a number of cases with success by the *continuous* galvanic current, and says that if *contraction can be obtained by it*, recovery is merely a matter of time, sometimes of months or even of years.

24. *Facial Neuralgia cured by the Constant Current.*—Two remarkable cases, most encouraging in their termination, are related by Dr. Wiesner as treated in Niemeyer's Clinic.:—

The first, a man, aged seventy-four, suffered for *twenty-nine years* from neuralgia of the three branches of the trigeminus. The constant

current was applied to the painful parts which were included between the moist positive pole and a metallic brush on the negative pole. Rapid improvement; after twenty sittings only a slight sensitiveness of the upper lip. A relapse some months after was also cured.

The second case was a man, aged sixty-four, suffering from facial tic for *five years*. Several branches of the fifth nerve had been divided, and even a portion of the upper jaw resected and the common carotid tied (!) with only temporary relief. At the time of the galvanic treatment, the pain was chiefly in the temporal nerve, and also in the infraorbital. The painful parts and those which were sensitive on pressure, were included between two moist sponge electrodes, twice a day for five minutes. Speedy improvement followed, but chequered by frequent relapses; however, on introducing one electrode into the mouth, the other being applied to a corresponding point on the cheek, rapid improvement ensued, and in two months all attacks of pain had ceased, and in three months the patient no longer remained sensitive to the influences which formerly excited the attacks.—(*Practit.*, March, 1869; quoted from *Berlin Klin. Wochens.* 17, 18, 1868).

25. *Continuous Current in the Treatment of Cardiac Paralysis, caused by Chloroform Inhalation.*—About two years ago, MM. Onimus and Legros published in the *Comptes. Rend.*, Mars., 1867, the results of their investigations on the treatment by constant galvanic currents of cardiac paralysis occurring during chloroform inhalation. The means generally recommended in the treatment of this accident, such as artificial respiration, flagellation, and aspersion with cold water exert but an indirect and insufficient action on the heart, and the use of interrupted electric currents though usually advocated and practised is still more objectionable, as both the respiratory and cardiac movements may be permanently arrested by such currents.

MM. Onimus and Legros adduced experimental evidence on this point which seems conclusive as to the necessity of employing the continuous current, and in a more recent paper in the *Gaz. Hebdom.*, they propose the following method of treatment:—"It suffices when the respiration has become arrested to apply the two poles of a constant *continuous* electrical apparatus, the one (negative) being placed in the mouth, and the other (positive) in the rectum; the respiration and circulation will immediately reappear."—(*Ed. Med. Jour.*, June, 1869).

PART IV.

MEDICAL MISCELLANY.

Reports, Translations, and Scientific Intelligence.

On the Relation between the Water of the Atmosphere and the Soil. From Prof. Fr. Pfaff, of Erlangen (Zeitschrift für Biologie, B. iv., H. 2 und 3). Translated from the *Ugeskrift for Læger*, March 13, 1869, Copenhagen; by William Daniel Moore, M.D., Dub. et Cantab.; M.R.I.A., L.K.Q.C.P.I.; Honorary Fellow of the Royal Medical Society of Copenhagen.

The communication formerly promised (Conf. *Ugeskrift for Læger*, Jan. 30, 1869, p. 79, and *Dublin Quarterly Journal*, May, 1869, p. 501) on the above subject, has now been published in the *Zeitschrift für Biologie.* Its principal contents are as follow:—

Observations on the relation between the water of the atmosphere and the soil, have hitherto been undertaken only with a view to investigating the chemical action of the water on the upper portion of the crust of the earth, chiefly in the interest of agriculture. The author has, on the contrary, proposed to examine the physical condition of the water in the soil, and especially to determine the quantity of atmospheric water penetrating to different depths, compared with the amount of rain-fall.

The rain which reaches the surface behaves in a twofold manner. One portion of the water penetrates the soil, supplying springs and wells, and playing the most important part in the nutrition of vegetables. The remainder in part flows off from the surface to form brooks and rivulets, in part returns, by evaporation, to the atmosphere. But it is only the water penetrating the soil which is of importance in the present inquiry.

To determine the amount of this water at different depths, the following experiments were undertaken:—Four tubes of tinned iron, respectively of six inches, one foot, two feet, and four feet in length, and six inches in diameter, were buried in an open flat place in a garden; they were filled to the brim with the dug up earth, and were furnished, at the depths stated, with a perforated bottom, from which the penetrating water was received in a reservoir, which could again be emptied through a well adapted lateral tube. All vegetable growths were kept away from the tubes, and they were kept constantly quite filled with earth, so that the rain could not remain standing on the surface. At the same time, the rain-fall and evaporation in the locality were accurately determined. It was a dry sandy soil, with a scanty growth of grass.

The experiments were continued for a year, from December, 1866, to December, 1867. The results were as follow :—

1. *The total quantity* of the water which penetrated in the first three tubes amounted respectively to 50·07, 51·26, and 60·81 per cent., that is, to rather more than half the total rain-fall of the year ; it appeared, at the same time, *that the quantity of water increases with the depth.* This holds good, however, only for the lesser depths, as the quantity again diminishes at the depth of four feet.

2. With respect to the *distribution of this total quantity according to the seasons* there was a very great difference. In the winter half-year (from the 21st of October to the 22nd of April) three-fourths of the rain-fall penetrated at least two feet into the soil, and there was scarcely any difference whatever in the quantity of water which percolated the several tubes. This is a confirmation of the old statement, that it is chiefly the moisture of winter which penetrates the soil. In the summer half-year (21st April to 21st October) there was an enormous difference, as only 7, 9, 32, and 18 per cent., respectively, sank down into the four tubes. It was, therefore, in the summer half-year that the greatest difference at the several depths appeared. At a depth of two feet, four-and-a-half times as much percolated the soil as at a depth of six inches (in the first tube). In the whole of two months (from the 27th of May to the 24th of July) not a trace of water collected in this tube, although the rain-fall amounted to 92 millimètres ; while in the interval between the 21st of October and the 2nd of December, with a rain-fall of only 51 millimètres, the percolated water amounted to 40·3 millimètres.

At a depth of two feet the percolation ceased only twice (in June, and in the end of September) ; at a depth of four feet it never entirely ceased, though, on the whole, it was less than at the depth of two feet.

Notwithstanding that these facts, at the first glance, seem rather strange, a closer examination will readily throw light upon the principles on which they depend. They depend upon the three following factors :— 1. The hygroscopic power of the soil ; 2. The evaporation from the surface ; 3. The character of the rain-fall.

1. *The power of the soil to retain the water* depends upon its composition ; thus, as, in the present instance, it was formed chiefly of quartz sand, it presented a network of capillary tubes, of calibre varying according to the size of the individual granules, and, therefore, more or less adapted to retain the percolating rain. At the same time, the individual granules are surrounded, through adhesion, by a thin layer of water, even where the distance between them is too great for capillary attraction to act. In addition, there is the tendency of the other constituents to combine with water, as is the case especially with clay and organic substances, the latter particularly on the surface. The earth with which the author made his experiments could thus, when perfectly dried, take up more than 20 per cent. of water, without the least quantity passing through. Even if,

on account of the greater density of the earth, the amount of water taken up be actually rather less, it will, nevertheless, always be considerable, and this shows satisfactorily how it is that after long-continued drought even a great deluge of rain may be retained in the upper crust of the earth (the vessels at six inches and a foot).

2. *The evaporation from the surface* is in summer very great; it is, therefore, dusty soon after even the heaviest shower. The amount of the evaporation in the deeper layers is, however, more difficult of determination. That it constantly proceeds deeper and deeper is shown by the fact that wells and springs are dried up in long-continued drought. The less the water-combining capacity of the soil, the more porous the soil; the greater its power of conducting heat, the more rapidly will the evaporation proceed from above downwards. If the upper layers are dried up, the water evaporates from the lower, but in these the evaporation goes on much more slowly. A part of the ascending vapour is again condensed in the upper layers, in summer, particularly at night; hence, it follows that the upper layers also dry more slowly when the soil is loose at a greater depth. These facts explain why more water percolated through the longer tubes than the shorter, especially in summer. For, as the earth contained in the tubes was completely shut off from the watery vapours ascending from the surrounding earth, it must dry up the more quickly, the thinner the layer was (in the short tubes). In any rain, therefore, the earth contained in the tube must have been completely saturated with water, before any percolation to the receiver underneath could occur; consequently the short tubes often for a long time yielded no water in the receiver, while the deeper, if complete exsiccation had not taken place, still afforded considerable quantities. The same circumstances also give rise to the great difference between summer and winter. Thus, in winter the evaporation is very slight—the soil is scarcely ever dried, even only a few inches down. The quantity of the percolated water is, therefore, about the same at all depths (in all four tubes).

3. *The character of the rain-fall* is a very important element. Constant, though slight, rain yields greater quantities to the soil than heavy rain of shorter duration, even when absolutely more water falls in the latter than in the former. In a shower, in fact, the water cannot sink rapidly enough into the earth—a great quantity flows away on the surface. This is completely confirmed by the author's observations. Many very violent showers yielded nothing at all in the receivers, while constant slight rain allowed considerable quantities to pass through.

The results obtained are far from being unimportant, especially so far as they may afford an important basis for continued experiments in other places, and under other circumstances. It is only after such researches that we can see what is of general, and what of only local, importance.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

Dr. M'Clintock, President.

Aneurism of Abdominal Aorta.—Mr. William Stokes exhibited an example of aneurism of the abdominal aorta, which occurred in a man aged 42, that had been under his care in the Meath Hospital for some months at the close of the year 1867 and beginning of 1868. He had always been temperate, but was occasionally exposed to much hardship in his occupation as a cattle drover. Up to four years previous to his admission into hospital he had enjoyed perfect health. He first noticed pain in the left side; three months later it settled down in the back, and became of a boring character. It was paroxysmal, and preceded by numbness in the left arm. The paroxysms occurred about twice a day—sometimes at night, also. On examining the abdomen a large abdominal tumour was found pulsating forcibly, and a distinct fremitus was felt on applying the hand over it. A loud rough murmur was also distinctly heard on applying the stethoscope on the tumour. There was extreme irritability and violent action of the larger arteries. The femoral and temporal arteries were pulsating violently throughout until the man's death. The pain he suffered was extreme. During the paroxysms, which at first were only about two in the day, but which became not only more violent, but more frequent, as the case went on, he used to scream with agony, and large beads of perspiration burst out all over his body. The only thing they could do was to palliate these symptoms by the exhibition of opium; and here he might say, in passing, that in no case that had ever come under his notice had he seen such beneficial effects from the subcutaneous injection of morphia, as in this one. At first, when the paroxysm came on, a single injection of morphia, 10 or 15 minims, was sufficient, but as the case proceeded, and the paroxysms became more frequent and more intense, a much larger quantity was found necessary; and shortly before the patient's death a solution of morphia of the strength of 10 grains to the ounce was employed, an amount equal to one grain being used two or three times in the day. Things went on in this way for several months, but at last he began to sink; he became dropsical, effusion took place into the pleura and pericardium, and he died without any rupture of the sac having taken place.

On making a *post mortem* examination there was noticed very great hypertrophy and dilatation of the left ventricle. The aortic valves were insufficient, and were very much thickened. The other cavities of the heart were likewise enlarged, and the arch of the aorta was greatly dilated and atheromatous. This atheromatous condition of the vessel existed throughout its whole course. The lungs were healthy, but upwards of

30 ounces of fluid were found in the cavity of the pleura, and six ounces in the cavity of the abdomen. An aneurism, of the size of a small cocoa-nut, was found, arising from the posterior wall of the abdominal aorta, just below the origin of the celiac axis. The bodies of the third, fourth, and fifth lumbar vertebræ were partly eroded. The chief points of interest connected with the case were the complication with cardiac and aortic valve disease; the occurrence of ascites (rare in cases of abdominal aneurism); and the strong action of the arteries of the neck and extremities. It seemed to be analogous to the violent pulsation of the carotid arteries sometimes observed in cases of pericarditis.—*December* 19, 1868.

Aneurism of the Thoracic Aorta.—Dr. Hayden detailed the case of a man 35 years of age, of temperate habits, who was admitted to the Mater Misericordiæ Hospital on the 19th September last, under his care. He stated that about a year ago he was attacked with a pain shooting across the front of the chest, and extending downwards more or less on the left side. He struggled on at his business until the date of his admission. The pain had then become fixed in the left side, that on which he generally lay. The following was his condition on admission into hospital:—There was defective motion of the left side; there was perfect precordial clearness over that side, but a total absence of respiratory sound, whilst on the right side the respiratory and percussion phenomena were normal; on both sides there was vocal vibration; the pulse was 98, regular, and equal on the two sides; the pupils were also normal and equal; there was neither dysphagia nor dyspnea, nor any laryngeal symptom whatever; he had a very slight cough, which could scarcely be regarded as being of a troublesome character. On examining the man's chest he failed to discover any impulse, even at the acme of expiration; nevertheless, he considered the preceding signs justified him in diagnosing thoracic aneurism. It had been stated that in cases of abolition of the respiratory sound on one side, without modification of percussion resonance, the phenomena would be found due to one of two causes, in the great majority of instances—either thoracic aneurism, or the impaction of a foreign body in the air passages. There was no evidence of the latter, nor was there any history of a foreign body. The man went on improving. On the 31st October the physical signs were unaltered, and on that day he was discharged in much improved health, being able to take food, and to sleep well, and being entirely free from pain.

On the first examination of the man he was struck with the difference in the size of the temporal arteries on the right and on the left sides. On the right side it was tortuous, large, and pulsated strongly, whilst on the left it was scarcely to be felt. There was a corresponding difference in the strength of the pulsation of the common carotid on the two sides, but

no difference in the action of the subclavian or radial arteries. On the 28th November he visited the man at his lodgings. He found him in bed, to which he had been confined during the previous week, and was informed by his wife that during that time he had been troubled with a hoarse cough, accompanied with mucous expectoration, but no sign of blood. His pulse was now 108, and exceedingly weak. The upper part of the left front of the chest was dull, but the dulness was of a peculiar character. For two inches below the left clavicle there was muffled tympanitic resonance. From this level to the nipple, resonance was only comparative, but normal in quality, whilst lower down and behind it was perfect. Respiration was absent, with the exception of bronchial respiration between the scapulæ. The man's condition underwent improvement. On the 3rd December he was again admitted into hospital, and soon began to sleep well and to take food, and the symptoms he complained of during the previous week disappeared—namely, what he fancied to be an obstruction about the pit of the stomach on swallowing solids.

On the 15th of the present month he saw the man for the last time alive. He was then out of bed, and walking about the ward, and apparently, and by his own declaration, much improved in general health. There was a slight difference in the condition of his chest. The dulness was now complete throughout the left side, with a total absence of respiratory sound, with the exception of the bronchial respiration behind, already mentioned. There was now, also, abolition of vocal vibration, as complete as existed in cases of effusion into the pleura. There was no displacement of the heart, and the dulness did not transgress the middle line. On the evening of that day, while in bed, about half-past nine o'clock, he suddenly screamed, and then vomited a vast quantity of blood. He put out his hand for the basin, and fell upon the floor. He was immediately visited by the resident student, and was then pale and pulseless; a large quantity of blood was found in the basin and on the floor. About a quarter of an hour afterwards the patient died.

Dr. Hayden felt that the evidence afforded warranted him, not only in diagnosing aneurism, but in determining its precise origin and position. From the absence of laryngeal symptoms, it was clear that the left recurrent nerve was not implicated. From the absence of dysphagia, it was evident the æsophagus was not pressed upon—the left bronchial tube only was interfered with; and from these data it was easy to arrive at the conclusion that the aneurism must be connected with the descending portion of the arch, or the upper portion of the thoracic aorta, and had passed forward so as to press upon and occlude the left bronchus.

On making a *post mortem* examination, six or eight ounces of serum were found in the pericardium. The heart was flabby, with a good deal of superficial deposit of fat. The cavities were perfectly normal in size, and the valves sound. The left lung was in the third stage of pneumonia,

and sunk promptly in water. This was the case throughout, except at the anterior edge, which was in the second stage. The right lung was normal, except that compensatory emphysema had taken place to a great extent. The aorta was slightly atheromatous. The orifices of the vessels arising from the arch were normal, except that of the left carotid, which was very much smaller than that of the subclavian. He found a number of enlarged and solidified lymphatic glands, one of which lay over the root of the left common carotid, and compressed it nearly to occlusion, thus accounting for the feeble pulsation of the left carotid and temporal arteries, and the increased pulsation of those of the right side. Two inches below the left subclavian artery, there was a vast rent in the aorta, which led into a sac as large as a middle-sized orange. The interior of the aneurismal sac was lined by laminæ of fibrin of considerable firmness, except at one side, where the wall of the sac consisted only of the pleura forming the wall of the posterior mediastinum, and here a communication was established between the aneurism and the left bronchial tube, on the one hand, and the œsophagus, on the other. Leading from the aneurism into the left bronchial tube was a very considerable sized opening, with a plug of fibrin projecting into it. There was a still larger opening leading from the aneurism into the œsophagus, and what was curious was, that the mucous membrane of the œsophagus was extensively ulcerated or eroded, and in the fundus of this superficial erosion was an aperture leading into the aneurism, which would admit an ordinary-sized quill.

A question of interest in the case was, Did the aneurism communicate with the œsophagus and the left bronchial tube simultaneously, and from which did the fatal hemorrhage proceed? Seeing the state of the mucous surface of the œsophagus, one would be inclined to think the process of ulceration commenced in it. On the 28th of November the man complained of difficulty of swallowing. He could imagine that any solid food the man swallowed would, by repeated friction over the surface of the œsophagus corresponding to the projection of the aneurism, ultimately give rise to erosion.

It had been laid down as a law in physical diagnosis, to which there was no exception, that the total absence of vocal vibration, and of crepitus in any degree, forbade the diagnosis of simple pneumonic consolidation of the lung. To this law there must henceforward be admitted at least one exception, viz., when from the hepatized lung air is excluded by the occlusion of the corresponding bronchus.—*December* 19, 1868.

Mitral Valve Disease.—Dr. William Moore showed a heart taken from the body of a girl, aged twenty years, who was admitted into Sir P. Dun's Hospital in November last. She was jaundiced and dropsical; her respiration was 50 in the minute. She suffered from palpitation for the past twelve months, but for the past six she had been incapacitated

from earning her living. The menstrual function was wholly suppressed. There was dulness on percussion for an increased extent over the precordial region, and a strong systolic fremitus could be felt towards the apex of the heart, and over the same region a loud systolic bruit was audible, most intense over the apex, and to be heard very distinctly at the inferior angle of the left scapula. The treatment for the alleviation of the collateral symptoms was attended with relief; but this was temporary, as about the middle of December the dyspnea assumed the form of orthopnea, and profuse hemoptysis ensued, the dropsy and jaundice becoming more developed. At this stage, however, the fremitus completely disappeared, and the systolic bruit became so subdued that excitement was required for its development. She died on the 1st January.

The *post mortem* examination showed the heart to be generally hypertrophied, and the right auricle and ventricle dilated. There was no evidence of disease of the aortic valves, but the mitral orifice was contracted and of a crescentic form. This was due to ossification of the valves. The aperture was scarcely capable of admitting an ordinary-sized goose quill.

Dr. Moore remarked that this was a good example of constrictive disease of the mitral orifice, which at one stage was also regurgitant. The loss of the more important physical signs, viz., fremitus and bruit *pari passu* with the extension of the disease, was well marked, a phenomenon connected with valvular disease first observed by the late Dr. O'Ferrall, of this city.—*January* 9, 1869.

Softening of the Cerebellum.—Dr. Lyons said the morbid specimen which he now exhibited was taken from the body of a man sixty years of age. His history was briefly this:—He was a labouring man, working very hard, and occasionally used to take stimulants, two or three glasses of whiskey in the day. He appeared up to the time he was attacked to enjoy moderately good health for his time of life. On the morning of the 21st December he got up to go to his work as usual about six o'clock. He felt a considerable amount of pain in his head and some dizziness. He took a couple of glasses of whiskey and went to his work. Coming on twelve o'clock in the day he found himself unable to continue and then made an effort to return home, and on his way fell in Westland-row. He was taken up insensible, put into a cab, and brought to his home. Towards evening he rallied a little and became somewhat conscious; he spoke, but in a muttering manner. He was treated at home for five days, and was then admitted to the Whitworth Hospital. His state was then very peculiar. He was unable to stand, and yet it could not be said that he was paralysed, with the exception of the left side of his face, which was slightly affected, the mouth being drawn over to the right

side. When he was told to put out his tongue it was with some difficulty that he did so, and it went slightly to the right side. He could with ease lift his arms and legs; yet he could not stand, but with a very little assistance he was able to maintain the erect position. There was this peculiar feature in the case, that he retained a great deal of muscular power in his arms and lower extremities. He was wandering and partly delirious at times. He was unconscious, but when roused by being spoken to he muttered his name in a very unintelligible way, put out his tongue, and did what he was told. He made efforts to get out of bed for the purposes of nature, though he frequently passed both urine and feces in bed, and on one occasion he made an attempt to get up on his knees in bed to make water and was enabled with a slight amount of assistance to maintain himself in that position. His pulse was 80 and not remarkable in any way; the pupils were rather contracted, and the face had a dull confused expression. He continued very much in this state, sometimes making efforts to get out of bed and being very delirious, and on one occasion in an effort to get out of bed he fell over on his right side, and that seemed to be the direction on which he on most occasions fell. He cut his face and nose and disfigured himself otherwise in falling on two or three occasions. He became so unmanageable that it became necessary to put a straight-waistcoat on him, as well as to have him watched at night by special attendants. The case was a very peculiar one. He could not at times be got to speak, and at other times he muttered a few words in a confused way. He died on the 30th December.

Autopsy.—The calvarium was closely adherent to the dura mater, so that it could not be separated without doing violence to the brain until the latter was altogether removed. There was a slight amount of bloody tinge rather than extravasation over the surface of the brain. On making a section through the organ and exposing the lateral ventricles a very slight amount of increased effusion was observable, and neither on the anterior surface of the brain nor when the corpus striatum was cut through could anything be found to account for the remarkable train of symptoms during life. On turning up the base of the brain, however, there was found a very peculiar condition. He (Dr. L.) examined the vessels in the circle of Willis to detect embolism, but nothing of that nature existed. The arteries exhibited a certain amount of opacity, the result of a slight calcareous deposit. The one remarkable condition was the utter destruction of a considerable portion of the left lobe of the cerebellum. The grey matter was converted into a kind of soft magma, and its structure was almost wholly undistinguishable; while the deeper white matter of the organ was but little affected. On reflecting on what could have possibly been the source of this remarkable amount of change, the only thing that occurred to his mind was that there was probably a condition of

temporary or transient embolism in the vessels supplying this part of the cerebellum. Having regard to the symptoms during life and the want of co-ordinating power without any marked loss of force in the upper or lower extremities, he thought this suggestion explained the chief feature in the patient's condition during life.—*January* 9, 1869.

Pericarditis.—Dr. Jennings exhibited a specimen of "pericarditis."

The patient, a man aged forty-two years, of stout and muscular habit, and who had been admitted into the South Dublin Union on the 24th December, 1868, was suddenly seized with extreme collapse on the evening of the 27th, and when visited by the resident pupil was found lying on his back comatose, with livid and swollen features, and an almost imperceptible pulse. From this condition he was roused—and then only partially and for the moment—with much difficulty, replying to questions with slow and indistinct mutterings, and instantly relapsing into his former state of stupor. Under general and prolonged stimulation the heat of surface was somewhat improved, though no other change could be detected in his condition until the following morning, when it was remarked that his insensibility was slightly less profound, and that he answered more promptly and distinctly.

On physical examination, extreme dulness was found to exist on both sides of the chest, except over the upper and anterior part of the right, where coarse and puerile respiration was audible; no cardiac sounds could be anywhere detected, nor could any pulse be felt at either wrist. His face was still congested. From this time he gradually sank until his death, which occurred rather suddenly on the following day at three o'clock.

On *post mortem* examination the right pleural cavity was found nearly two-thirds full of serum, and the upper lobe of the right lung presented a puckered appearance, being also solidified throughout almost its entire extent. On section it presented a greenish-gray colour, was thickly studded with tubercles, and contained a few small cavities. The middle lobe also contained a few tubercles, but was generally crepitant. The upper portion of the lower lobe was likewise solidified and tubercular, the lower being still more solid and fleshy, and containing several patches of apoplectic effusion.

The left pleura was intimately adherent throughout its entire extent, and the upper lobe of the left lung was extremely solid and tubercular, as also the upper part of the lower, which, in the rest of its extent, was, however, more or less crepitant. The bronchial glands were enlarged and tubercular.

The pericardium was closely adherent to the surrounding parts, the visceral and parietal layers being separated by a layer of lymph, in some places nearly half an inch in thickness. This was in parts soft and

fleshy, and in parts closely adherent to the proper pericardium, which itself was very much thickened.

The effusion of lymph was exceedingly abundant about the base of the heart, completely concealing the origin of the vessels, and the auricular appendices. On the right side the valves were healthy. The walls of the left ventricle were soft and of a yellowish-brown colour, and apparently fatty. Both the mitral and aortic valves were healthy. The heart was apparently enlarged owing to the copious deposit of lymph on its surface. The external surface of the aorta was studded with numerous small extravasations, and the vessel, when opened, presented an extremely advanced state of atheromatous degeneration, the inner coat being thickened, wrinkled, and covered with spots of atheroma, and broad bony plates.

Dr. Jennings remarked that the case which he had just recorded presented many features of no ordinary interest, not merely as an instance of sudden death, which many might have attributed to cerebral agency, but from the strange latency of the symptoms, and the absence of suffering during life, a fact which was rendered still more inexplicable when it was borne in mind that the various morbid changes which had reached such a degree of development as to seem incompatible with the continuance of life, had undoubtedly been produced by repeated attacks of acute and intense inflammatory action. Nevertheless, neither had the patient made any complaint while under his observation, until immediately before his death, nor—so far as he had been able to ascertain—had he been at any time under medical treatment.—*January* 9, 1869.

TRANSACTIONS OF THE CORK PATHOLOGICAL AND MEDICO-CHIRURGICAL SOCIETY.[a]

Remarks on the Treatment of Zymotic Diseases with Sulphurous Acid. By W. Jackson Cummins, M.D., Physician to the Cork South Infirmary and County Hospital, &c., &c. Read March 24, 1869.

It is about four years since I first drew the attention of this society to the prophylactic treatment of zymotic diseases by sulphurous acid. At that time my clinical experiments had been made with the alkaline bisulphites administered internally, and after detailing them to you I summed up in the following words:—

"The very extreme of the importance of results such as these tends to cast a doubt on their reality; and, while I can assert that I have not overstated them in any particular, I must add that I accept the conclusions to be drawn from them with reserve, and look anxiously for their

[a] These Reports are furnished by the Secretary, Dr. Purcell.

confirmation in any trial which others may be induced to give the medicine on my recommendation." Now sir, the paper in which these experiments were published has been quoted, and favourably commented on in periodicals and medical works, but the practice has not gained ground, nor, as far as I know, been much adopted; this I am not surprised at, as to carry it out efficiently, requires an amount of close watching, and I may add enthusiasm, not to be expected from ordinary experimentalists. Indeed, I have myself, almost entirely given up the bi-sulphites as prophylactics since I discovered the much more efficient and practicable mode of introducing sulphurous acid into the system, by burning sulphur in the apartment of the sufferer, and allowing its fumes to be inhaled by him and his attendants.

Subsequent to the epidemic of cholera in 1866, I read a paper detailing a series of experiments made on that disease with the fumes of sulphur; at the time I believed my idea of thus attempting to limit the spread of zymotic diseases—an idea which grew out of my experience of the value of sulphurous acid administered internally—was exclusively my own; but shortly after my paper appeared, Dr. Pairman, of Biggar, corresponded with me and drew my attention to publications by himself and Dr. Dewar on the use of this agent in various diseases, on principles similar to those I had advocated. These works have been favourably reviewed in various periodicals, and attracted much public and professional attention. Since then Professor Lyster propounded his views on the treatment of wounds and abscesses by carbolic acid, used on similar principles; and within the last year much has been written on the antiseptic treatment of disease by these agents.

I come, therefore, before you now to make a short communication on the subject, fortified by the experience of other and more distinguished observers than myself.

Diphtheria is not a very common disease in Cork, but from time to time it startles us by sweeping away whole families in one fell onslaught. Such a calamity occurred about two years ago, and carried to the grave five members of one household. About the same time I was called to see a young gentleman, who had been suffering from sore throat for four days. I at once recognized it as a case of diphtheria and requested a consultation. Dr. Finn was called in, but the disease had progressed beyond hope, and in a few days terminated fatally. The family was a large one, and no precautions against spread had been used until my first visit; from that time, however, and for several weeks, sulphur was burned day and night in the patient's apartment and throughout the entire house. Before long the mother and grandmother, who attended the child through his illness, the servant who took care of his room, the brother who had taken his place in the bed where he had slept, and another child who had occupied the same room both before and after his removal to another, were attacked

and suffered from sore throat of a diphtheritic character; but in all the disease was so modified, I presume by the sulphurous acid, that it yielded to simple treatment. We cannot say positively that any of these cases would have presented more malignant symptoms if sulphurous acid had not been inhaled; but the contrast between the history of this family and of the other I have alluded to, as well as with many similar instances which from time to time have been related before this and kindred societies, admits the belief that the antizymotic had done good service.

Since last autumn scarlatina has been more or less prevalent in my district.

I kept no note of the cases, but should say that they numbered over thirty. Sulphur was by my directions burned in all the houses, and all the cases terminated favourably except one, a child who appeared to be convalescent when I ceased to attend him, but died unexpectedly a week after. I also paid two visits to a girl attacked by vomiting, collapse, and sore throat, who died in a few hours without any attempt at rally. I presume it was a case of suppressed scarlatina but, cannot remember whether sulphur was burned or not, as it was a private case which I only saw incidentally without being acquainted with the family before or since.

Within the last two months there has been a new outbreak of scarlatina at the other side of my district, and, contrary to its wont, the disease prevailed as much, perhaps more, among the better classes than among the poor. A few of the cases have been severe, but the majority of a mild type. In all the houses of my patients, whether poor or rich, I have had sulphurous acid set free day and night, and the effect of its inhalation upon both the general condition and the throat has been most manifest. Within twenty-four hours in every case the unhealthy dusky ulcerations of one day have been replaced before the next by healthy clean-looking sores, which rapidly cicatrized in a few days. No case has proved fatal, nor have any sequelæ occurred, except mild arthritic swellings in two instances. Convalescence has been rapid in all, and return of strength extremely rapid.

The ordinary treatment of these cases was most simple—generally a mild emetic of ipecacuanha at first, a few doses of chlorate of potash, perhaps a dose or two of hyd. c. creta, and in the worst cases claret and beef tea from an early period.

It is only right to mention, however, that, except in one instance, the few cases treated by other physicians in the district recovered also. But the most remarkable effect of the antizymotic treatment was the comparative immunity enjoyed by other members of those households which adopted it, whole families having escaped infection in some instances, and in all, except one, the subsequent cases being much less severe than the first.

It may be supposed that the almost continuous smell of sulpurous acid kept up in a respectable house for weeks together would be found intolerable, but such is not the case; if set free in too large a quantity at a time, it of course produces cough, sneezing, &c., &c., but burned frequently in small quantities, it is not found disagreeable, and from the sense of purity it imports to the air it is rather pleasing to some people, myself among the number. If I remain long in the sick room when sulphur is burning, I find that it leaves a sweetish taste in my mouth, and invariably gives me a feeling of hunger and extreme relish for food. The nurse of one of my little patients, who being an asthmatic, was apprehensive as to the effect of the fumes upon herself, informed me subsequently that her chest was much better than usual during the time the smell of sulphur was kept up in the house.

I cannot conclude this communication without quoting extensively from a paper on the pathology of cholera by Dr. E. A. Sansom in the February No. of the *Medical Press and Circular*, as much of the information there compiled to illustrate the pathology of that disease is applicable to all zymotic diseases, and to the subject before us.

It may seem incredible to a superficial observer that a small portion of sulphurous acid diffused through a sick room could influence the progress of diseases, the outward manifestations of which are often so appalling; but when we consider the extreme minuteness of the molecule, possessed of vital properties, which introduced into the blood multiplies thereon, it becomes more intelligible, thus to quote from Dr. Sansom:—"The investigation of the phenomena of vaccination has shown how almost inconceivably minute are the particles capable of inducing disease. Not only are these said to be, under the highest powers, molecules scarcely visible, but the researches of Chauveau have shown that if vaccine lymph be diluted with 150 times its bulk of matter, and a milligramme of the solution be conveyed on a lancet-point to an animal organism, it is capable of inducing the phenomena of vaccine disease. According to Beale the organized bodies capable of producing cattle plague must be less than 1-50,000th part of an inch in diameter. Analogy, therefore, lends colour to the hypothesis that zymotic matter (and by inclusion cholera matter) is composed of particles at the utmost verge of vision;" and again Dr. Farr, quoted by the same author, says—"The habits of the vibrion, the velocity of its development, and its power of disorganizing other organic forms, are seen under the microscope. If vibrions constituted the cholera ferment, diffusion through water, and to some extent through sewers or air . . . would be perfectly intelligible. Two vibrions would become by fissiparity 4, 8, 16, and at the 100th generation, at this uninterrupted rate, 126,765,000,000,000,000,000,000; and as generations succeed each other by the hour, this accounts for the swarms in every drop of cholera fluid."—(Report, cit. p. lxvii.)

Dr. Sansom says further, "In a solution in which spontaneous fermentation has not yet begun be examined under the microscope, besides the organic *debris* nothing can be discovered. Soon, however, appear granules, and afterwards vibrios, It follows that there existed before these vibrios evident to vision, germs of vibrios which were not evident to vision;" and again, "Dujardin discovered fifty species of infusoria during the putrefaction of a single substance. Moreover, the forms presented belong not only to the vegateble, but also to the animal world. Mitscherlich long ago propounded the doctrine that animalculæ were amongst the essential agents of fermentation. It is certain that the development of microzoa accompanies that of microphyta;" but not only are innumerable and low forms of organized beings produced and reproduced from a single parent of inconceivable minuteness, but the pabulum from which such beings are reproduced has an influence on the form and structure of the new beings, for Hallier states "that certain fungi appear in distinct, definite, and unvarying forms, accordingly as they are cultivated in a soil rich in nitrogen or in a soil poor in nitrogen."

In my communication on cholera, I dwelt upon the influence of the pabulum on which the zymotic poison reproduces itself although unaware at the time, that such had actually been demonstrated; and this knowledge makes intelligible the varied type and intensity presented by the same disease in different persons and different epidemics.

It is unnecessary for me to comment upon the bearing of the quotations which I have made upon the antizymotic and antiseptic treatment of the class of diseases, for it must be apparent to all how an agent such as sulphurous acid, which is admittedly destructive to low forms of life, can extinguish the germs of disease, and thus become prophylactic, or even curative of the early stages of illness and of secondary inoculation at later periods.

In a conversation which I had with Dr. O'Connor a short time ago, he suggested that morbid poisons may have the power of reproducing themselves in rooms, fomites, &c., &c., and although I can hardly suppose that organic poisons can be reproduced, except from an organic pabulum, yet experience and experiment both favour the idea that poisons which undeveloped are harmless, may undergo in sick rooms and fomites the development essential to their potency. The following, taken from the author I have so largely quoted, demonstrates the correctness of this opinion. "The synthetic test has been applied first by Thiersch, secondly by Sanderson. They administered in minute amounts the alvine discharge of cholera patients to healthy mice. The result was the production in many cases of choleraic symptoms in the animals. The experiments which showed communication of the disease were made at a mean temperature of 56° F. At a temperature of 49° F. they failed. If we can accept

with any definiteness the results of these inquiries we may deduce some further information concerning the cholera germ. According to these, as tersely summed up by Dr. Farr, "The fresh fluid the first day after exposure to the air is almost inert, on the second day it grows more active, on the third it is at its maximum of activity, is less and less active on the fourth and fifth, inert on the sixth day of transformation.' It thus appears that the greater or less morbid power of the cholerad coincides with the varying phases of its development."

Jeremiah Connolly, a pensioner, and employed as a porter of late, was admitted into the South Infirmary, under the care of Dr. Wm. C. Townsend, on the 30th December, 1868.

The patient was thirty-eight years old, and unmarried; had served some time in India as a soldier; was troubled with a tape-worm, which was removed from him in 1859; was pretty healthy, but got a severe cold about nine weeks ago, and a cough; found a great difficulty in expectoration; vomited constantly; and lost his appetite; was seen then and sent to the Fever Hospital by Dr. Wm. Townsend, where he remained a fortnight; did not feel quite recovered at the time he left it, and in a short time his old symptoms gradually crept on him again, and in the end became so troublesome that he had to go into hospital.

When he came into hospital he was dreadfully weak and emaciated, and could not make use of any food; presenting all the appearances, as he lay in bed, of a patient in the collapse of cholera; complained of a constant sickening pain about the stomach, vomited some grumous matter; mixed with a lot of stringy mucous. His skin was cold and rough, and had a peculiar bronzed appearance. He had slight clubbing of the fingers.

On percussing the chest, there was dulness over the apex of right lung; the stethoscope discovered bronchial breathing, and the sounds of the heart were hardly perceptible; his pulse very feeble.

On the night of his admission got two aperient pills. Next morning, gr. iij. of quinæ sulph. in a glass of sherry twice a day, and he was supported with beef-tea, chicken broth, milk, brandy. While he was in hospital was gradually getting weaker, and vomited four times up to the morning of the sixth, when he had a very severe attack of retching, which lasted the whole day, until about half-past four on that evening, when he died.

Post mortem appearances.—The brain was slightly congested; some tubercular infiltration in superior lobe of the right lung; heart atrophied, but healthy; liver and spleen both enlarged. The coats of the stomach thin and showing some patches of inflammation. The right kidney was hypertrophied; the left one atrophied, and presenting a shining bluish black appearance. Both supra renal capsules were a mass of scirrhus.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

THIRTY-FIRST ANNUAL SESSION.

The Seventh Meeting of the Society was held in the College of Physicians on Saturday evening the 8th May. The Chair was taken at half-past eight o'clock, by

DR. RINGLAND, President.

DR. KIDD, Honorary Secretary, read the minutes of the last meeting.

The Chairman said, in the absence of Dr. Beatty, who had moved the adjournment of the debate, it would be resumed by the seconder, Dr. Johnston; but Dr. Kennedy had asked to be first allowed to make some observations in explanation:—

DR. KENNEDY said the president had been good enough to ask him to give a *résumé* of his paper, but he thought that unnecessary, as it had now been for two nights under discussion, and the paper itself was in the hands of the members. There was, however, one point in which he wished to set himself right before the discussion was proceeded with. In the statistics which he furnished, he gave those of the Vienna Hospital only up to 1838—the only statistics which were then available to him; but, on looking over the *Quarterly Review*, he found that Professor Vay, of Copenhagen, mentioned that there had been an improvement in that hospital, and he thought it his duty to lay that statement before the Society; it was simply a hearsay statement. He gave no statistics, but merely stated that it was one per cent. If he had known this before, he (Dr. Kennedy) would have stated it, but he now gave it for as much as it was worth.

DR. JOHNSTON said:—

Mr. President and Gentlemen—We must all allow that Dr. Kennedy deserves credit for the great pains he has taken, and for the no small amount of labour and trouble he has devoted in searching out and calculating statistics, in order to bring forward arguments against the continuance of Maternity Hospitals on their present arrangements. But I think it can be shown that the statements he has promulgated are based on wrong hypotheses, and as his allusions are more particularly directed against the hospital that I have the honour of management, I shall principally confine my observations to the state of that institution.

Allow me, in the first instance, to remark that I cannot see exactly the grounds upon which Dr. Kennedy has stated, in his answer to the Medical Commission, that "although continued fever, typhus or erysipelas, are prevalent in medical and surgical hospitals," it is a necessary conse-

quence that "puerperal fever should prevail in the Lying-in Hospital." Nor can I conceive that if such an epidemic should be prevalent in the city generally, there is any reason to say, because a patient was admitted to an hospital labouring under one or other of these diseases, as has frequently happened, that the institution should be charged with being the *habitat* of such a poison, or that it in any way engendered it.

Nor can it be admitted that "this zymotic disease (puerperal fever) prevails *endemically* in hospitals generally," although we have no hesitation in allowing that it may appear occasionally in those which may be "ill-ventilated or over-crowded." And I think it far from correct, nay, I say it is an injustice to put forward the dogma that "where puerperal fever is epidemic, it shows itself generally in our great maternity hospitals *in the first instance*." Such, I maintain, is not the case. On the contrary, how frequently have we known of its prevailing in other quarters amongst patients confined in their own homes, and even among the affluent, when the hospital was perfectly free from any sickness.

When we read from Dr. Kennedy's paper that "a malady thus zymotic in its type and origin is produced by a poison emanating from parturient women, &c., but not requiring more than one parturient female to generate it, when the poison she herself has generated may, as in the case of blood-poisoning, be reabsorbed into her own system, and self-contamination then as certainly strike her down as if a crowded, ill-ventilated lying-in ward was the generating medium." And then when we consider that in the over-crowded, unwashed, ill-ventilated rooms, where this poison finds a fertile soil, and where, no doubt, many deaths occur from this malignant poison that we never hear of, that it is from this hot-bed the great majority of those come who seek the advantages of an hospital, can it be wondered at that occasionally cases of metria should be seen within our walls.

Contrast those hovels with the large, lofty, and airy wards of our institution, where perfect thorough ventilation is observed by outlets on all sides, and where the patients are not crowded together, but ample cubic space of air allowed for each, so as so completely to dilute this "poison" emanating from the parturient women as to render it perfectly innocuous. Where the strictest cleanliness, by scouring and fumigating, by that attention to the patient in sponging and daily change of clothes; where the ejecta are at once removed, thereby preventing that lochial effluvia which is so objectionable; where every attention is paid to the wants and well-being of the patients.

It is difficult to understand, nay, I maintain, where such rules are strictly enforced, it is hardly, if at all, possible that malaria miasm or any poison could find "a *habitat*."

I may be answered by the observation, "Wait till you have a little more experience of the hospital. Look at the statistical reports."

Well, on looking over them, I find that during Dr. Collins's mastership the death-rate diminished considerably, owing, no doubt, to the successful steps he took to lessen the mortality by improving the condition of the atmosphere by ventilation, &c.

Dr. Shekleton, during his mastership, in the same way, had the same happy results. Since then, through the exertions of Dr. M'Clintock, and afterwards of Dr. Denham, a more thorough system of ventilation has been introduced, thereby increasing the ingress of fresh and a freer egress of foul air, and thus tending to render the hospital as healthy as one could almost desire. I therefore consider that I would be wrong in not giving my contradiction to the assertion made by Dr. Kennedy in his letter to the Governors, viz.—"That the intention of the founder, in relieving the miseries of all the lying-in women who have applied for admission, has failed, &c., &c., simply because the original plan and construction of the hospital was *faulty*," &c., &c.; that the "congregating a number of lying-in women under the same roof engenders and spreads amongst them a disease *sui generis*, and of the most fatal character." And as this is not the only letter that has been written, and as observations have been made detrimental to the hospital, I think it incumbent on me to describe here the exact position and plan of the establishment. The building, having a north and south aspect, stands in an open space, its front to south receding from the street 80 feet, which is at least 60 feet wide, thus being distant from any house fully 140 feet. It has the large space of Rutland-square at its rere, without shrub or tree within at least 100 feet, except one (an ash) at its west end, planted by the late Dr. Collins, but not interfering in any way with the flow of air. A corridor on each floor, 9 feet wide, runs from east to west the whole length of the building, in the direction of the prevailing winds, with windows at either end having the upper part of them open, so as to admit a constant and thorough draught of air. Off these corridors on either side are the wards, two at either end of each, averaging a superficial space of 34 × 24 feet, and 13 feet high, which, if I may so say, do not adjoin one another, having a wide space intervening, that on the south occupied by the Chapel, and on the north by a broad staircase with large window (which is constantly open), thus isolating the wards as much almost as if they were separate buildings. There are circular openings at each end of the ward opposite to the fire-place; there are large elliptical windows near the ceiling in the wall opening on the corridors, opposite the windows, three of which are in each ward on the north, two on the south, besides ventilators running to the roof. The amount of cubic space for each individual, calculating at five in each ward, would be at least 1,800 feet; but whenever we can, if the labour as we say is slow in coming in, we limit the number to four in each ward, thereby, of course, increasing the cubic space, and by so doing maintain a perfectly pure and innocuous atmosphere, and I have every reason to

think that such an object has been attained. For I may certainly say that anything like miasm or poison does not exist, and can confidently affirm that nothing approaching to contagion has appeared. No doubt we have had deaths from metria, but in almost all instances they were either victims of seduction, sent for the most part from the country away from their friends, to hide their shame under the shadow of the hospital, where among the many they might pass unnoticed, or others labouring under great distress of mind, without home, deserted by cruel, hard-hearted husbands. And it is a remarkable fact, and worthy to be noted, that where a patient was attacked with the malady, it was confined to that particular individual, never extending beyond her.

Even in Ward 11, where Dr. Kennedy remarks that during his mastership it was so liable to miasm and poison, that he was obliged to close it for several months, we had proofs of its present healthiness. As instances, in November last two patients were admitted from the same locality, both having dry tongues, with brown fur, pulse 100 ; both were delivered naturally, but shortly after their confinements they each showed symptoms of peritonitis of the same type. Both were treated in the same way ; one died, the other recovered, nor did any other patient in the ward take the sickness. All went on favourably through their puerperal week, and were discharged perfectly convalescent.

A case in 7 ward, 59 bed, in December, second child. Symptoms of erysipelas set in immediately after labour. We had her moved at once to a small ward. She died nineteen days after delivery. All the rest in the ward, four in number, made excellent recoveries.

A case in 12 ward, 111 bed, in January. Sent from country, the victim of seduction, who never held up her head from the time she came in ; fretting extremely. Pyemia attacked her after confinement, from which she died, yet no one in the suite of rooms took it—all went on well.

How, with such instances as these, can the hospital be charged with being the "*centre of malaria*," or how can any one assert that we "*delude poor women*," or that the hospital offers anything else but a safe asylum to those seeking its relief in their hour of trial and distress?

In alluding to the enormous death-rate in the hospital, if Dr. Kennedy will only take a little further time to consider, and when I bring to his recollection that this hospital, possessing such a world-wide fame, receives, without let or hindrance, *every patient* seeking relief in her hour of trial, coming from all quarters of England, Ireland, and Scotland; or some, who having been under the care of unskilful practitioners outside, who when finding, after a long and lingering labour, that they cannot accomplish the delivery, and when even they have allowed the uterus to rupture under their hand, send them in this hopeless state to the hospital ; when we receive creatures unmarried, sent up from the country

and elsewhere, and spurned by their relatives—suffering both in mind and body—who have no other place to seek shelter but the hospital, and who may happen to succumb, it may be from metria, all of which cases we must account for in our statistics. If we remove these from the category, our death-rate would not appear so frightful as Dr. Kennedy has made it out, nor should the hospital get the credit of being the "*hotbed*" for the cause of it.

Dr. Atthill said:—

Having for several years past paid great attention to the subject treated of in Dr. Kennedy's valuable paper—a subject the importance of which it is impossible to over-estimate—I wish, briefly, to make a few remarks on two of the propositions he lays down; but first I desire to state that I entirely agree with some of his conclusions. I believe metria to be often epidemic, under certain circumstances contagious, and to a great degree preventible; but for the very reason that I believe this proposition to be true, the greater is the necessity for carefully investigating the question in all its bearings, and taking every care to avoid false premises, and erroneous conclusions. Now, the two of Dr. Kennedy's propositions to which I take exception are—1st, that any parturient female may generate and absorb a poison capable of producing metria; and 2nd, that the generation and absorption of the poison of metria is in a direct proportion to the number of puerperal females "cohabiting" at their parturient period, or who breathe the same atmosphere when lying-in.

Now, I cannot but entertain great doubt as to the soundness of the first of these. If it were true, I think a larger proportion than occurred in Dr. Kennedy's own practices, viz., 1 in 1,200, would have generated, and absorbed this poison; indeed the weight of evidence is against this proposition, but I do not consider it a point of real importance. We know, with positive certainty, that the contagion emanating from such diseases as erysipelas, scarlatina, &c., will originate metria in puerperal females, and it adds nothing to the weight of the arguments adduced in favour of a modification of our system of lying-in hospitals, that the disease is capable of self-generation; but I shall not waste time in discussing a question which is merely speculative, and I should not have alluded to it at all, were it not that we run the risk of losing sight of more certain and far more important sources of infection by attaching undue weight to it. I believe it will be admitted that metria is *generally* due to influences acting on the patient from without, even by those who hold the theory that it may be self-generated.

There is, probably, no one who has been for any time engaged in obstetric practice, who has not seen metria resulting from the infection of such diseases as erysipelas and scarlatina, &c., indeed Dr. Kennedy

alludes to this, but a still more fatal cause of the disease has not been alluded to by him, and one to which hospital patients are specially liable, namely, contamination from the hands of the attendants themselves.

I need hardly remind this meeting of the remarkable result of the investigation into the causes of the mortality in the Vienna Lying-in Hospital, published by Dr. Arneth in the year, 1851. For ten years prior to the year 1847, the mortality of that hospital had ranged from 11 to $5\frac{1}{4}$ per cent., but in that year extraordinary precautions were used to prevent the male attendants, who were all engaged more or less in dissecting, and in making *post mortem* examinations, from entering the wards till their hands had been thoroughly cleansed and disinfected; the result was that the mortality fell at once to $1\frac{1}{4}$ per cent. in 3,526 deliveries, and has continued at a comparatively low rate ever since. This is a striking instance of one of the causes of metria, and, at the same time, a proof of Dr. Kennedy's assertion—that it is a preventable disease. And, I think, that until all possibility of infection from this source is carefully excluded, we cannot rightly estimate the effect of such assumed influences as self-generation, or generation in consequence of aggregation.

I must also point out that Dr. Kennedy has fallen into an error with respect to the returns of the Vienna Lying-in Hospital. He quotes those of the year 1838, giving the mortality at 4 per cent., whereas it should be about $1\frac{1}{4}$ per cent.—this being the mortality for the last twenty years. But this leads me to the second proposition, which I propose alluding to, one from which I entirely dissent; namely, that "the generation and absorption of the poison of metria is in a direct proportion to the number of parturient females cohabiting in their parturient state." I look on this proposition as not only contrary to what is generally known of the disease, but as being directly disproved by the very statistics on which Dr. Kennedy relies; first, let me again refer to Dr. Arneth's work, I have just now alluded to the very remarkable results of the precautions adopted in the Vienna Lying-in Hospital. But what was it that led the physicians of that institution to suspect that it might be the hands of the pupils, who were engaged in anatomical studies, which gave origin to the disease? It was this—that in the portion of the hospital which was devoted to the education of female pupils, which was under the same roof with the other, which received the same number of patients, and in which the patients were treated in all respects alike with those in the other division—save that they were attended by female instead of male pupils—the mortality was comparatively small, the deaths being but 30, against 600 in the male side. Surely this one fact is totally incompatible with the theory "that metria is generated in a *direct proportion* to the number of parturient females cohabiting during their parturient period," or that this is *the* cause of the disease.

But now let us turn to the statistics given by Dr. Kennedy, and we shall find them to be altogether contrary to his theory. First, let us look at those years in which large numbers of patients were delivered in the Dublin Lying-in-Hospital. The days are long gone by since the annual deliveries within its walls reached 3,000. But what was the mortality in those days? Why, it was low. We find such low mortality as 1 in 309, 1 in 205, 1 in 220. Nor is this rate of mortality confined to isolated years, which could be explained away by saying it occurred during exceptionally healthy years. But take five consecutive years. Thus, in the years 1805 to 1809, in which there were 12,691 deliveries, the mortality was 1 in 157—a low mortality when it is remembered how large a number of cases are annually sent into the hospital merely because they are "bad cases," among which the mortality must be great. And I take this opportunity of protesting against the fairness of comparing the statistics of an institution which gives free admission to the very worst cases, even though, as frequently occurs, they be moribund at the time of admission, and the great majority of whose patients are always from the very poorest class, with those of our private patients. Now it so happens that during the years 1800, 1, 2, 3, 4, being the five years which immediately preceded those whose statistics I have just quoted, the annual deliveries averaged 500 less than the succeeding ones, being in the five years 8,990. Yet the mortality was more than double, being 1 in 67. Turn now to later periods. Take any five years you choose during the last 30 years and you will find the rate of mortality further increased, though the deliveries were not within one-third of those already quoted; and, finally, to come to very recent years, when the annual deliveries have fallen to some 1,200 a year, the mortality is still higher. Now, with these facts before us, will any one continue to maintain that metria is the result of the mere collecting together under one roof of a number of puerperal females, and "that it is in a *direct* proportion to the number of females cohabiting together during their parturient period, or who breathe the same atmosphere when lying-in;" and are we not really injuring a good cause by basing our arguments on such erroneous grounds? I wish to say again, I endorse Dr. Kennedy's assertion that metria is often epidemic, under certain circumstances contagious, and to a great degree preventable. One other fact I would point out, namely, that the lowest mortality which has been in the hospital for twenty years past was during the four years from 1850 to 1853, when the deliveries were far more numerous than in later years. From the foregoing facts I think we may, without hesitation, conclude that the generation and absorption of metria bears *no relation whatever* to the *number* of parturient females collected together during their parturient period. We must, therefore, admit that to epidemic influences, and to causes acting from without the patient's own body, the increased mortality from this malady

is due; for I suppose no one is inclined to assert that our skill in treating disease is less than it was fifty years ago. What, then, can be those causes? They are probably numerous. I point out one—namely, that which was proved to have been, in the case of the Vienna Lying-in Hospital, so fruitful a source of this deadly malady, namely, the hands of pupils, who have been recently engaged in dissecting, or in making *post mortem* examinations, and which have been but imperfectly disinfected. But it may be asked—was not this cause in existence fifty years ago? I answer, yes—but only to a very limited extent, both because at that time the study of midwifery was much neglected, and few students availed themselves of the advantages afforded by the hospital, and also because, from the paucity of subjects, the facilities for making dissections were greatly curtailed. Here, then, we have a cause, clearly proved, and preventable, and one which certainly should not be lost sight of in a discussion such as this. There are other causes, too, equally preventable, which I could point out; but I do not wish to occupy unduly the time allotted to this discussion.

The following is an abstract of the foregoing statistics:—

Deliveries in the Dublin Lying-in Hospital, during the three quinquennial periods,

	Deliveries.	Deaths.	Ratio.
1st.—1800 to 1804 inclusive,	8,990	134	1 in 67
2nd.—1805 to 1809, „	12,691	81	1 in 157
3rd.—1864 to 1868, „	5,758	175	1 in 31

The mortality being in an exact inverse proportion to the deliveries.

Dr. T. More Madden said:—

Mr. President,—The cure and prevention of puerperal fever, which are the subjects now under the consideration of the Obstetrical Society, are questions the importance of which it would be difficult to estimate too highly in a society such as this. And as one of the medical officers of the Dublin Lying-in Hospital—the statistics of which have been so largely quoted by Dr. Evory Kennedy in his paper on puerperal fever, the great interest of which I fully appreciate—I desire to take some part in the present discussion, and to record some facts within my own experience, which is however very limited when compared to his, in which my observations lead me to form opinions widely different from Dr. Kennedy's views.

If the facts be as Dr. Kennedy believes them to be, his conclusions are undoubtedly right, and in that case no one would more earnestly agree with him than I should, that it is our imperative duty to lose no time in exposing and remedying whatever faults in our lying-in hospital system have occasioned the disastrous results which he and many others—though none more ably—attribute to the present system of lying-in hospitals.

But I think I can convince even Dr. Kennedy, that as far as regards the hospital with which I have the honour to be connected, at least, his views require reconsideration.

Speaking of the great lying-in hospitals of Europe generally, Dr. Kennedy says—"Be it recollected these poor women flock to those hospitals under the impression that they are gaining a safe asylum in their hour of trial and distress; little do they imagine that they are, in their ignorance, taking a step that adds to their risks of death in a ratio at the very lowest calculation of three to one, and at the highest of twenty to one against their lives." Applying himself to the lying-in hospitals of Dublin generally, Dr. Kennedy says,—"The result would be that in the City of Dublin alone seven and a half women die out of every nine from being confined in hospitals; in other words, that in all the deaths that have occurred in Dublin for the last seven years in parturition, out of every nine deaths seven and a half women have died who would in all human probability be at this moment alive had they been confined in their own homes or in isolated cottage hospitals." And speaking of the present death-rate of the Rotunda Hospital calculated on the average of the last 15 years, Dr. Kennedy says, it "is about one in thirty," and he adds, "comparing the epochs the death-rate increases in a fearful ratio." I purpose, however, to endeavour to prove that the present death-rate of the hospital is not so high as Dr. Kennedy thinks it is, and that instead of increasing the death-rate is actually decreasing in the Rotunda Hospital. Before doing so I will, with the permission of the Society, first make a few observations on some points referred to in this paper.

With respect to the treatment recommended by Dr. Kennedy in cases of puerperal metria, I certainly think that the views of so able a physician, and one who has had such a vast experience of the treatment of the disease as a consulting physician, and as Master of the Lying-in Hospital, are entitled to the highest respect, and his opinion as to the necessity for administering stimulants freely in that malady is quite consonant with the experience of the majority of modern midwifery practitioners. But still I must say that I cannot agree with him as to the necessity for local depletion in cases of puerperal fever. He says,—"In true puerperal fever of the present day the use of the lancet is rarely admissible; local depletion must be our sheet anchor. But in having recourse to it the secret of success is to reduce the pain by repetition of leeching before reaction has had time to establish itself." With very few exceptions all the cases of puerperal fever I have seen were essentially of an asthenic type, presenting all the symptoms of so-called malignant puerperal fever, or those of puerperal pyemia, and consequently were not suitable cases for any depletion (though I have seen leeches used with advantage in cases of inflammatory puerperal peritonitis), on the contrary the cases I now speak of required the free administration of stimulants and nutriment.

The medicines I have seen do most good in cases of puerperal fever were turpentine in drachm doses, with mild sedatives, and chlorate of potash. The symptoms being, of course, treated as they arose in each particular case. I do not agree with Dr. Tyler Smith that drugs are useless when once pyemia sets in; for I have attended cases of puerperal pyemia which recovered under treatment, but I have not as yet seen much benefit derived from the use of the sulphites recommended by Drs. Polli, Sanson, and other recent writers, still my experience of their use is too limited to enable me to speak with confidence on this point.

Dr. Kennedy tells us that puerperal fever prevails endemically in crowded hospitals, and that when epidemic it generally shows itself in these in the first instance; and there can, I think, be no doubt that from the outbreak of the first well authenticated epidemic of puerperal fever, which took place at Paris during the winter of 1746, where the disease proved most fatal to those confined in the Hotel Dieu, down to the present day, the proportion of deaths from this cause has always been greatest in lying-in hospitals. But this is, I think, to be in a great measure attributed to faults in the construction, ventilation, hygienic condition, and management of these hospitals, which faults do not, I contend, now exist in the Rotunda Hospital. I have visited a great many hospitals in almost every part of Europe, and very recently I have devoted some time to going through several of the London lying-in hospitals, and I may venture to say I have never been in any hospital where more scrupulous attention is paid to cleanliness, ventilation, and hygienic condition than is the case in our lying-in hospital. Besides this the disease often prevails out of doors when the hospital is perfectly free from it; and I have seen instances of this within the last six months. The question then mainly turns on the hygienic condition and ventilation of the hospital, and if these be perfect, as they should be, and if sufficient cubic space be allowed to each patient, the mere number of parturient women confined together can matter comparatively little.

There can be no doubt whatever that puerperal fever is a contagious disease, and that it may be carried by an intervening person from one parturient woman to another; and we can never take too much care by availing ourselves of all the precautions which experience can suggest to prevent the possibility of ever being the unconscious bearers of the fomites of this dread malady from one patient to another. The necessity of this precaution being sadly proved by the mortality from this cause which has followed in the track of some practitioners. But, although I fully admit this, still I do not think that contagion is the only or even the general way in which this disease, as an epidemic, is spread. On the contrary many facts have been recorded long ago by Dr. Hulme, Mr. Hey, of Leeds, and M. Tonnelle, as well as by Dr. Snow Beck, and Dr. Brunton, of London, recently, which tend to throw much

doubt on the question whether puerperal fever is so peculiarly contagious as it is commonly supposed to be or not? I have now seen a good many cases of puerperal fever, and I never yet saw two patients, lying in adjoining beds, affected with it at the same time. Though I have seen patients in different wards, or even in different parts of the same ward, suffering from it at the same moment.

The truth is, I believe, that puerperal fever, like all other zymotic diseases, is not spread by contagion alone, although it is undoubtedly contagious, but it is also, and I am convinced more generally diffused by what the old writers with great propriety termed "the epidemic constitution of the atmosphere." In other words, that the disease is diffused by a peculiar atmospheric poison which develops a disease that may be communicated by elimination from those first infected by it to others. And the weight of evidence I think should lead us to believe that, as I once endeavoured to prove when speaking of another zymotic disease, contagion has probably much less to do with the spread of puerperal fever than the peculiar epidemic constitution of the atmosphere by which the disease is carried from place to place.

Dr. Johnston has called your attention very forcibly and very truly to a consideration of the highest importance in estimating the causes of what Dr. Kennedy thinks the excessive mortality of the Lying-in Hospital. And the point is one so essential to be borne in mind in this discussion, that I cannot pass it by in silence, even though I may to some extent repeat what Dr. Johnston has already urged much more ably than I could ever hope to do. It is, I think, the proudest boast of the managers of the Dublin Lying-in Hospital that they never refuse admission to any parturient woman who seeks shelter within its ever-open portals in her hour of labour. Within these walls, I may again venture to remind the Society, are received not only the wives of the poorest class of the labouring population of this city, but also the most destitute and most wretched of all God's creatures—the deserted, outcast, friendless, homeless victims of seduction, who flock hither, not only from every part of Ireland, but moreover, as our registry proves, from every quarter of Great Britain. These are the patients who most frequently succumb to puerperal fever in the hospital. In their case the pangs of labour are assuaged by no moral consolation; the present is full of anguish and the future is lighted by no ray of hope. Can we wonder that these poor creatures, predisposed to disease by the combination of every moral and physical cause which could depress their vital powers, indifferent to life, nay, even, as I have but too often myself heard them, wishing for death—is it, I again say, to be wondered at that under such circumstances they should fall ready victims to puerperal metria? And is it then just to reproach the hospital with this?

Those who say that by continuing to receive patients into the wards of our

large lying-in hospitals we must only insure the deaths of many parturient women who would otherwise have been spared to their families, seem to ignore the vast number of lying-in women who are carried into such an institution as the Dublin Lying-in Hospital as a *dernier recours* in cases of preternatural and difficult labour, and also of the thousands who, had they not received the comforts of a lying-in hospital, must have perished at home from sheer destitution at this time. If the specific zymotic poison of puerperal fever be generated in consequence of overcrowding hospitals, as it might doubtless be, is it not also likely to be generated in the over-crowded garret of the poor? I have myself visited a lying-in woman in a room so crowded that each inmate was allowed, as I found on measurement, rather less than 250 cubic feet of space. Surely this woman was much worse off in a hygienic point of view than she would have been in the clean well-ventilated wards of the hospital, where every patient has considerably over 1,500 cubic feet of space to breathe in. And yet such are the places into which, if the Lying-in Hospital be suddenly closed, the poor will be driven in their hour of trial, for it will naturally take a long time to supply the place of these great hospitals by new plans of attending these cases. Nor would I venture for a moment before a Society such as this, every member of which is so fully acquainted with the circumstances of the sick poor of this city, to dwell on the scenes of almost incredible destitution and misery to which I have sometimes been summoned to attend lying-in women, who did not avail themselves of the advantages of the noble institution founded by the charity of Bartholomew Moss.

In speaking of the rate of mortality in the Rotunda Hospital, Dr. Kennedy says—"But in stating the gross mortality of the Dublin Lying-in Hospital now as 1 in 72, I should lead you into an error, as you will perceive, by this table of the annual mortality given from its foundation, that for the last 15 years it has never been less than 1 in 64, and in that it has in one year risen to 1 in 14. The average of these 15 years is about 1 in 30, and this is the state of things we have now to contend with—one sufficiently grave to command our immediate attention." . . . . . "And we must not conceal from ourselves," adds Dr. Kennedy, "that in comparing the epochs, the death-rate increases in a fearful ratio."

Now, confining myself entirely to the period during which I have been connected with the Lying-in Hospital, I can show that our present death-rate is much lower than Dr. Kennedy's calculation, based on the average of the last fifteen years, leads him to think it is, and it is still decreasing every quarter. Thus, from the 4th of May, 1868, when I was elected Assistant Physician to the hospital, to the 4th of May, 1869, I have taken notes of 1,132 cases of labour, which took place in this institution. And I find that the proportion of deaths recorded from

any of the diseases which Dr. Kennedy included under the name of zymotic metria, is very small indeed, amounting to sixteen in all, or rather more than one and a half per cent. During the same period twelve other deaths of adults occurred in the hospital, but in these are included the deaths in the chronic ward as well as in the lying-in wards. The causes of death in the cases which did not result from zymotic metria were

From Cancer,	2	deaths.
Accidental hemorrhage,	2	,,
Bronchitis,	2	,,
Shock after delivery,	2	,,
Puerperal Convulsions,	1	,,
Disease of the heart,	1	,,
General dropsy,	1	,,
Ovarian dropsy,	1	,,
Total,	12	

The causes of death in the cases recorded from any zymotic puerperal disease were:—

From Puerperal Fever	6	deaths.
Metro Peritonitis	3	,,
Pyemia,	3	,,
Erysipelas,	2	,,
Phlebitis,	1	,,
Not stated,	1	,,
Total,	16	

Within the last few months the death-rate from zymotic puerperal disease in the hospital has fallen. Thus, from the 1st of January, 1869, to the 4th of May, 1869, inclusively, I have recorded the particulars of 403 deliveries, and I only find mention of five deaths from metria, and one of these took place on the 1st of January, so that in reality there were only four deaths from metria in these 403 deliveries, or about one per cent.

This I think is a just ground for congratulation to those who believe in the benefits of large lying-in hospitals, and who are of opinion that it would be very difficult to close them in great cities such as this, by substituting a new system of medical attendance for puerperal women, without causing great inconvenience not only to the pupils, who are now instructed in the art of midwifery in these institutions, but what is of infinitely greater importance, much serious inconvenience for a long time to come to the poor women who now find an ever-open and comfortable asylum in these great hospitals in their hour of labour. At any rate, though we differ in

many respects from Dr. Kennedy's views, we must all admit the importance of the subject and the ability with which he has treated it, and although we do not quite agree as to the means, yet I am sure our object is identical—namely, the relief of suffering humanity.

We have not, it seems to me, at all sufficient data in the tables which have been brought under the notice of the Society by Dr. Kennedy on which to rest a fair comparison between the results of home attendance and those of hospital attendance in midwifery cases. The circumstances of patients confined in the small cottage hospitals in country districts, which have been contrasted with the Dublin, London, and other great lying-in hospitals, are not analogous to the circumstances of the patients received into the large maternity hospitals of great cities. Moreover, even if the change suggested were desirable, the obvious practical difficulties in the way of founding small cottage hospitals in great cities would be enormous. No argument has been adduced against lying-in hospitals which does not apply with greater force against every other class of hospitals, medical as well as surgical. Are we, however, on the mere faith of a few statistical tables, which, ably as they may be put together to sustain certain views, are confessedly based on insufficient data, to do away with all the great hospitals of Europe; to shut these "palaces of the poor," as the hospitals have been well termed by a celebrated physician; to turn the poverty-stricken victims of illness in our crowded cities away from the noble legacies left to the sick poor by the Christian charity, and, I will venture to add, by the wisdom of our ancestors? When we are prepared to do this, as, indeed, some eminent authorities do not hesitate to say we should, then, but certainly not till then, should the lying-in hospitals be closed.

We would all do well to bear the fact in mind how apt we are to exaggerate the importance of subjects which we have expended much time and labour in elaborating, and to estimate the value of novel views that have been adopted by us by the exclusive, and, perhaps, undue attention we have devoted to them.

Dr. Kidd said, before the discussion went further, he would, with the permission of the Society, supply some statistics of the Coombe Lying-in Hospital for a period of fifteen years, which were asked for by Dr. Kennedy at the last meeting. It would probably be in the recollection of many members of the Society that the return with which Dr. Kennedy had been supplied was for a period of seven years, and that, instituting a comparison between that return and those of other hospitals, he expressed his regret that he had not a return for the whole period of fifteen years. He (Dr. Kidd) now held in his hand a return for that period, prepared by Dr. Ringland, and without going into details he would mention the leading facts:—

RETURN *from Coombe Lying-in Hospital, Dublin, for Fifteen Years, from* 1854 *to* 1868 *inclusive.*

Year	Number Delivered			Deaths			Death-Rate		
	Prior to end of 7th month	From end 7th month to full term	Total	From all forms of Metria	From all other causes	Total	From all forms of Metria	From all other causes	From all causes
1854	11	421	432	5	1 Placenta previa, 1 Ruptured uterus,	7	1 in 84·200	1 in 210·500	1 in 60·143
1855	13	413	426	3	2 Convulsions,	5	„ 137·666	„ 206·500	„ 82·600
1856	9	502	511	4	——	4	„ 125·500	No death,	„ 125·500
1857	18	520	538	3	1 Post partum hemorrhage,	4	„ 173·333	„ 520·000	„ 133·000
1858	14	413	427	4	1 Convulsions, 1 Hemorrhage,	6	„ 103·250	„ 206·500	„ 68·833
1859*	8	270	278	0	0	0	No death,	No death,	No death,
1860	10	437	447	6	1 Convulsions, 1 Mania, 1 Typhus fever,	9	„ 72·833	„ 145·666	„ 48·555
1861	7	346	353	2	1 Ruptured uterus, 1 Mania, 1 Syncope, 1 Bronchitis, 1 Convulsions,	7	„ 173·000	„ 69·200	„ 49·428
1862	10	503	513	5 Puerperal fever,	1 Mania, 1 Hemorrhage,	7	„ 100·600	„ 251·500	„ 71·857
1863	20	465	485	3 Peritonitis,	2 Debility, from starvation, 1 Admitted dying, sank 3 hours after admission, 1 Convulsions,	7	„ 155·000	„ 116·250	„ 66·429

RETURN *from Coombe Lying-in Hospital Dublin, for Fifteen Years, from* 1854 *to* 1868 *inclusive—continued.*

Year	Number Delivered			Deaths			Death-Rate		
	Prior to end of 7th month	From end 7th month to full term	Total	From all forms of Metria	From all other causes	Total	From all forms of Metria.	From all other causes	From all causes
1864	15	506	521	1 Peritonitis, 1 Puerperal Fever,	1 Convulsions,	3	„ 253·000	„ 506·000	„ 168·666
1865	15	435	450	2 Peritonitis,	1 Peritonitis, of which she was dying when admitted, 1 Bronchitis,	4	„ 217·500	„ 217·500	„ 108·750
1866	21	442	463	4 Peritonitis, 2 Phlebitis,	1 Typhus fever when admitted, 1 Cholera, 1 Ruptured uterus, 1 Convulsions,	10	„ 73·666	„ 110·500	„ 44·200
1867	11	422	433	4 Peritonitis, 2 Phlebitis, 1 Pleuritis,	1 Typhus fever when admitted, 1 Bronchitis, 1 Placenta Previa, 1 Convulsions,	11	„ 60·286	„ 105·500	„ 38·363
1868	21	478	499	1 Peritonitis, 2 Phlebitis,	1 Syncope, 1 Chronic Bronchitis,	5	„ 159·333	„ 239·000	„ 95·600
Total,	203	6,573	6,776	55	34	89	„ 119·509	„ 193·323	„ 73·854

* Hospital closed for repairs during a great portion of this year.

The Return made at the request of Dr. Kennedy was for the last seven years of the above period, namely, from 1862 to 1868 inclusive. The total number of Deliveries during that period, excluding 113 premature births entered to the end of the seventh month, amounted to 3,251 of this number 28 patients died from some one or other of the several varieties of metria, and 19 from other causes, making a total mortality of 47 ; the death rate being accordingly 1 in 116·107 from metria ; 1 in 171·105 from other causes ; and 1 in 69·170 from all causes. The 15 years included in the above Return, give 6,573 births, exclusive of 203 antecedent to end of seventh month ; of this number 55 died in the Hospital from metria, and 34 from all other causes, making a total of 89 deaths, the death rate rate being 1 in 119·509 from metria, 1 in 193·323 from other causes, and 1 in 73·854 from all causes. During those 15 years, 15 patients, labouring under various forms of fever, were removed to other hospitals ; and whilst it is known that some of those recovered, the history of the majority is unknown. In order, however, that there may be no under calculation on the point, it is assumed that all have died ; which would raise the number of deaths from metria to 70, and the total deaths to 104. The death rate on this estimate would be 1 in 93·043 from metria, 1 in 193·323 from other causes, and 1 in 63·202 from all causes.

During these 15 years the total number confined in the Coombe was 6,573—that was of patients confined at full term. All cases of abortion and of premature labour taking place before the seventh month had been excluded. Of these 6,573, the total deaths amounted to 1 in 73·854. Of these the deaths from puerperal fever were 1 in 119·509, and from other causes 1 in 193·323. During these years 15 patients were removed from the Coombe Hospital in consequence of being affected with some form of infectious disease, and brought to other hospitals. They had not been able to trace the history of those patients; 13 of them left during the first period of 7 years; 2 left in the latter period of 7 years. He believed, indeed, in the ninth year of the period of which he spoke, and for the last six years, no patient had left on account of being affected by any infectious disease. They had not been able, as he had said, to trace the history of these 15, but they knew that they did not all die, and they also knew that they were not all affected with puerperal fever. But suppose they granted that the whole of these 15 died, then their average mortality from puerperal fever would be 1 in 93·043 instead of 1 in 119·509, and their total mortality would be 1 in 63·202.

The consideration of the cause and prevention of puerperal fever had been very ably brought before the Society by Dr. Kennedy; and he expressed his own feelings, and he was sure those of the Society, when he said that they were greatly indebted to him. It was due to the Society that it should be brought forward. It was a subject which was being discussed throughout Europe. There was no capital in which it had not attracted the attention of the profession; but in no more able hands could it have been placed than in Dr. Kennedy's. But with all respect to him he (Dr. Kidd) thought he had been too exclusive in his views. He argued on the assumption that contagion was the great cause—he might almost say the exclusive cause—of puerperal fever. He (Dr. Kidd) thought that view too exclusive; and he thought if they adopted it they were sure to commit serious and grave errors. There could be no doubt that, as Dr. Johnston had put before them, the causes of puerperal fever were often individual—causes belonging to the patient herself. Distress of mind, as had been well shown by Dr. Collins, was a fruitful source of puerperal fever. No patient succumbed to it so rapidly as the woman who had been seduced. The patients who were subject to distress, starvation, misery in all its forms, were the most liable to it; and they were the patients to whom the hospital was of the greatest use—those who most required the shelter of its walls. Then they had epidemic causes producing it. The return of the Rotunda Lying-in Hospital, extending over a great number of years, was in itself sufficient to prove that there was some great epidemic influence at work, else how could they account for it, that during three

years of Dr. Collins's mastership they had it very prevalent, and that during the succeeding four years they scarcely had it at all, and then Dr. Kennedy, who had been Dr. Collins's assistant, and had helped to carry out the reforms that he instituted, became master, and again the fever became prevalent. Was it because Dr. Kennedy took less care than Dr. Collins? was it because the contagion was allowed to form a centre whence to spread in the hospital? He was sure that was not the case; and they must look for some other cause. This applied to all the hospitals of which they had a history. They found that for a period puerperal fever was at a minimum, and then from causes that could not well be traced, it rose suddenly and rapidly to a great extent. He believed it was due to some epidemic constitution of the atmosphere, the nature of which they did not yet know. Again, the form of puerperal fever varied. In no two outbreaks of it would they find exactly the same form. How were they to account for that? Could they believe that it was because the contagion varied, or that there was some difference in the condition of the atmosphere? He believed that that was the explanation of it. It was a long time since they had in the Coombe Hospital a case of puerperal fever in which the joints were affected. Some years ago they had it in almost every case. Lately they had no local disease; no peritonitis, no effusion into the joints, no inflammation of the pleura, or any of the other great serous cavities. Their patients at once sank into a typhoid condition, in which they died from asthenia, and without any local cause. But let it not be supposed that when he spoke in this way of individual causes of the disease and of epidemic causes, he for one moment doubted the influence of contagion. On the contrary, he believed contagion was a most influential cause of it; that the disease was highly contagious, and that if they were to prevent it they could only hope to do so by directing their attention to this. Dr. Madden had told them just now that he never saw two patients ill of puerperal fever in adjoining beds. That was not their experience at the Coombe. They had there had cases in adjoining beds. They had one remarkable instance where they had a case of puerperal peritonitis in which they thought it wise to apply leeches. This woman got erysipelas, and recovered, while the patient at each side died from puerperal fever, in one case accompanied by effusion into the joints, and in the other the patient died before that could have taken place.

The statistics of extern practice and of lying-in hospitals had been compared. If the results could be depended upon lying-in hospitals ought to be closed at once and for ever. But could they be depended on? He had taken an active part for twenty years in the management of extern practice in connexion with the Coombe Hospital, where they had always had a large extern practice. They had taken as much pains as they could to have the returns accurate, but he was sorry

to say he did not believe the returns could be relied on. From the nature of things he thought it impossible that the returns of extern practice could be relied on—at least in the same way as those of the hospital. Besides, the connexion of an extern practice with an hospital influenced the mortality returns of both, for where they met with a deformed pelvis, or with a case of convulsions, or with anything rendering the case difficult among the externs, they at once removed the patient to hospital. Their return of mortality was not, therefore, to be taken as a fair return of the mortality belonging to the hospital itself, nor to the extern practice. Dr. Atthill had put forward, in a paper published a few months ago, some comparisons on this subject. Who was there that, having a case of placenta previa, would trust her to extern practice? Yet there were no cases more fruitful of mortality. But notwithstanding all this he must admit that there was a large proportion of mortality due to the influence of the hospital. He believed it was impossible to collect a number of patients under one roof, and yet not to have an hospital atmosphere, and that that atmosphere was more or less injurious. During the past few years this subject had engaged their attention very much at the Coombe. They had been contemplating the rebuilding and enlargement of the hospital, and they had therefore paid not a little attention to the mode by which this could be best obviated. Of the different plans that had been suggested for the prevention of this influence, they had not neglected the experience of the Vienna Lying-in Hospital, but he did not attach importance to the remarks of Dr. Arneth with regard to the cause of puerperal fever. It was quite true that when Dr. Semelweiss caused the hospital to be divided into two branches—one attended by females and the other by males—that for a number of years the mortality became exceedingly small in that attended by the females, and remained at the usual average, or even greater, in that attended by the males. But it was stated at that Society some years ago by Dr. Denham, on his return from the Lying-in Hospital of Vienna, that during the past few years the mortality had been reversed, and that the mortality in the branch attended by the females was very much larger than in that attended by the males. That disposed of the charge of dirty habits that it was sought to fasten on the students, and he did not think the Dublin students were liable to the accusation which Dr. Semelweiss brought against those of Vienna, that they proceeded from the dissecting-room to patients without washing their hands. Another matter that arrested their attention in connexion with the hospital was the idea of isolating their patients. It was the proposal that Dr. Kennedy had put forward. When he spoke of isolating the patients he meant it literally, by having a separate ward for each patient. The plan had been tried in the Hospital of St. Petersburgh, which in 1852 had been reformed by the Grand Duchess Helena.

It was increased by the erection of a new wing of the figure of the letter L on the corridor plan. Opening off the corridor were a number of small wards in which the patients were placed. But what had been the result of that? The reform was instituted in 1852. The mortality for the seven years preceding it was 3·12, and for the seven years succeeding 2·93—that was to say, before the patients were placed in separate wards 1 out of 32·17 died, and that after this change—after they had gone to all this expense and trouble, and more or less injured the efficiency of the hospital, the only change was that 1 died in 34·09. He did not think that was an inducement to go to the expense, and to lessen the efficiency of their hospital by attempting to have separate wards for each of the patients. There remained another expedient, and that was to isolate their sick patients, and that was the plan they were about to adopt. The plan which they proposed to carry out, the results of which remained to be seen, was that they should have three separate and distinct buildings. The present hospital will form one wing. In it there are two large wards lighted by windows along one side and at one end, with a door always open at the other end. They would have another building corresponding to this, but separate from it by a considerable space, and having no connexion with it except through the open air, and there they would have a similar arrangement. Then they had in contemplation—and this part of the building he hoped to see commenced before many days—to have a detached house at the rere, to which they could carry any patients who became affected with the symptoms of puerperal fever. This would be an extension of the system on which they had been working for many years. As soon as they saw a patient manifesting any signs of puerperal fever they emptied the ward. They could not send the patient home nor use the pole-axe, so they sent her for some time to Cork-street Fever Hospital; but the difficulties attendant upon this were so very great that they were obliged to give up the practice. They now sent out all that were healthy, and shut up the hospital until the patient was removed—until she recovered or died; and they did not allow any other patient into it until it was thoroughly cleansed and fumigated. They proposed to carry this principle further. If puerperal fever manifested itself in one wing they would shut it up and work the other. He thought they were indebted to Dr. Kennedy for having brought this subject before them. In common with his colleagues, he looked forward to the result of that discussion with great interest, because of the contemplated change in their hospital; and if it should enable them to derive any hints as to the better arrangement of their new buildings it would be received with very great satisfaction.

Dr. Mapother said he would not intrude upon the meeting, but that all the previous speeches had been on the side opposed to Dr. Kennedy, and

he therefore thought this a convenient opportunity for making any remarks upon the subject which occurred to him. He did not think that any of the speakers had controverted the remarkable statistics which had been brought forward by Dr. Kennedy, with regard to the comparative mortality in the Dublin Lying-in Hospital, and that in the small provincial towns—Waterford, Limerick, and New Ross. He agreed with Dr. Kidd that the statistics of out-door practice were unreliable, and that a fair comparison could not be made between the lying-in hospital practice and the out-door practice, but the remarkable contrast which existed between the Dublin Lying-in Hospitals and those in the provincial towns was worthy of the very greatest consideration. The only other point upon which he would make an observation was with reference to puerperal fever or metria. He had for many years believed that it did not enjoy any separate entity, that it was merely the infection of many other diseases given to persons much more liable to the spread of such disease and to its fatal effect than other individuals. A great deal of albumen existed in the blood of the parturient female, and he had no doubt that the excessive fatality of women at that period, by zymotic disease, was owing to this circumstance. He had seen in the St. Vincent's Hospital many cases of erysipelas which had come from the Rotunda Hospital during outbreaks of puerperal fever.

Dr. Churchill said:—

Mr. President,—If I did not feel that it would be a cowardly shrinking from duty, I should very much prefer listening to speaking this evening.

The subject is one of supreme importance, and, I think, involving much greater difficulty than has been supposed; and, moreover, it has been brought before us with all the weight of great experience and the skill of a leader in the profession. I can claim neither such experience nor such weight of personal character, and, therefore, I propose to consider the question mainly from the facts collected and published by others, with the few I can contribute myself.

On so important an inquiry it is essential that we should divest ourselves of preconceived opinions, and that our conclusions should be rigidly limited to the facts they cover. We are, in fact, occupied in what I may call a judicial investigation of the greatest moment—nothing less than whether we ought not to reverse all our hospital arrangements, and, confessing the evil they have done, enter upon a new and opposite course. Have we data of sufficient weight and accuracy to justify this? That, I take it, is the point to be considered this evening.

But before entering directly upon the main question, there are one or two conclusions in Dr. E. Kennedy's paper which, I think, require a little modification or limitation, and one of which has a very distinct bearing upon the problem we are to try to solve.

After stating that puerperal fever is due to the absorption of a poison, Dr. K. adds:—

"2. That this poison may be generated by any parturient female; and where the circumstances are favourable to its imbibition it may be absorbed into the system of the generator or that of any other parturient female exposed to its influence.

"3. That the generation and absorption of this contagion is in a direct proportion to the number of parturient females cohabiting at their parturient period, or who breathe the same atmosphere when lying-in.

"4. That in lying-in hospitals, where large numbers of patients are delivered under the same roof, this disease finds its habitat, appearing and reappearing at uncertain intervals.

"5. That its appearance, although apparently capricious, is not infrequently traceable to the occurrence of other zymotic diseases, to a general unhealthy state of the hospital, the labours for some time being succeeded by bad recoveries before the true zymotic metria exhibits itself."

And, on the negative side, he observes:—

"9. Zymotic metria is not a disease peculiar to parturient women confined in their own houses, occurring comparatively rarely among them.

"10. It is not a disease observed to occur in small lying-in hospitals or cottages where only one or two patients cohabit in their lying-in."

1. Now, it does appears to me that too much stress is here laid on the spontaneous generation of puerperal fever in hospitals, and, notwithstanding the concluding paragraph, that there is hardly a due estimate of the epidemic influence as a cause. I do not deny that a systematic neglect of sanitary precautions in a hospital might originate puerperal fever as well as other diseases; but this charge cannot be maintained against the Rotunda Hospital, and yet we know that at uncertain and irregular intervals it appears in that hospital without anything unusual in its condition or in the condition of the patients to account for it. If it be produced spontaneously, why do long periods of freedom from it occur under precisely the same hospital conditions as those which accompany its appearance. In his account of an epidemic of puerperal fever in 1845, Dr. M'Clintock observes:—"During the months of January and February, 1845, the wards were remarkably healthy, insomuch that no death occurred out of the 367 women who were delivered within this space." And yet, between the 2nd and 5th of March 14 cases occurred of the worst form of puerperal fever. Whatever part contagion may have played in this part it cannot alone account for it. For this reason I believe, that whilst it may originate, like any other fever, under bad sanitary arrangements, it is really epidemic, and so propagated to a much greater extent than by contagion.

2. I hope that Dr. Kennedy will allow me to add a slight qualification to No. 9 and 10 of his negative conclusions.

Certainly zymotic metria is not peculiar to women confined at home, but assuredly they are not exempt from it. In a report of the Western Lying-in Hospital for 1835–6, I find, that although we had not a single case in the hospital, 11 cases occurred among women delivered at their own homes; and a similar circumstance is mentioned in connexion with the Glasgow Lying-in Hospital. How could poor women escape, with all their disadvantages, when we find that the rich, with all their appliances, do not. Dr. Evory Kennedy met with three fatal cases in private, Dr. M. Clarke with six, and I have seen six.

3. Now, as to its occurrence in small lying-in hospitals, I may mention that in the Western Lying-in Hospital in 1838 2 fatal cases occurred; in the Montreal Hospital, 5 cases; in the Waterford Hospital, 2 cases; in the South Eastern Hospital, 11 cases; in the British Lying-in Hospital, 7 or 8 cases occurred. When we remember how few were the cases annually delivered in these hospitals, not only must we hesitate to assume the absolute immunity of small hospitals, but, for the same reason, *i.e.*, the small number of deliveries, the proportion of puerperal fever appears even considerable. On the other hand, I think it cannot be doubted that a large hospital, however well arranged, with many patients, however well cared for, will prove an admirable hotbed for intensifying and propagating the disease, *when once it appears there.*

4. The next conclusion which, I think, requires some limitation is the one referring to contagion:—"That it is produced by contagion, long experience proves, following in the steps of certain practitioners, whilst others are totally free from it, and that in the same locality." I do not question that puerperal fever may be both generated and propagated by contagion. Dr. E. Kennedy's pupil "on the pale horse" may be an example. In Dr. Campbell's case, from his own confession, there can be little doubt; nor, in the case of the Manchester nurses, or the pupils of the Vienna Lying-in Hospital, who, according to Dr. Semelweiss, were in the habit of going straight from the dissecting-room to the obstetric wards of the hospital.

But to do this it requires favourable circumstances, *i.e.*, personal uncleanness, abstinence from water, from clean clothes, and fresh air.

Dr. E. Kennedy is himself a proof that, with due care, the disease is not conveyed by medical men. Having myself practised precisely the same precautions as those recommended by Dr. Kennedy, I can honestly say that I never had the slightest reason to suppose that I had carried it, although my number of fatal cases in private is double his. If this danger be so great as has been stated, not only ought every medical man attending puerperal fever to refuse attendance on fresh cases, but

no one liable to be called to a midwifery case ought to aid another in consultation; yet I never heard of this precaution being thought necessary.

Yet it is true, however inexplicable, that in a few instances, puerperal fever has dogged the steps of one medical man when others in the neighbourhood have been exempt. And if I am asked to explain this, I must simply avow that I cannot. If you inquire minutely into some of these series of cases, you will find that in some the succession was broken by considerable periods of time, and in others by the intervention of deliveries not followed by puerperal fever. This is as difficult to explain on the theory of contagion as the fact I have just stated is on the epidemic theory. You remember the case of the unfortunate man described by Dr. Meigs, who had thus a succession of cases. In despair he left the town, took warm baths, got new clothes, shaved his head, and after a fortnight in the fresh air in the country he returned, and the first case he attended died of puerperal fever.

I have dwelt the longer upon this latter point because I know that the assertion some time ago, by high authority, that puerperal fever was generally carried by the doctor caused great distress to patients and great injury to accoucheurs.

After these preliminary remarks, I come now to the question of the *comparative* safety, and, therefore utility, of out-door maternities, small lying-in hospitals, and large hospitals, and I feel that each step ought to be taken with great care and caution.

For solving this problem we are entirely at the mercy of statistics, and without undervaluing them—few have made more extensive use of them—I feel that they may easily be taken for more than they are worth.

I would remark, that to be of any authority they must be very extensive; in fact, their value is exactly in proportion to their extent. Let me illustrate my meaning by a few examples.

Dr. E. Kennedy gives only the number of deaths from metria 1 in 1,200.

In Dr. M. Clarke's private practice of 44 years, in 3,816 deliveries there were 22 deaths, or 1 in 173.

In Dr. Crosse's, in 1,377 cases, 14 died, or 1 in 98.

In Dr. Labatt's (50 years), 4,368 cases, there were 26 deaths, or 1 in 168.

In the private practice of a London accoucheur, referred to by Dr. M'Keevor, in 2,982 cases there were 30 deaths, or 1 in 99.

In my own, in 2,548 cases there were 16 deaths, or 1 in 159.

Would any of these suffice for a standard of the mortality of private practice? If all were added together they would afford no sufficient ground for estimating the death-rate after confinement. If indeed they were multiplied by 20, 30, or 50, we might have some confidence in the conclusions drawn from them.

And the same rule applies to small hospitals in ordinary times, but with tenfold more force when an epidemic of puerperal fever occurs. In a small hospital or private practice you may go on for years without a death, yet after the lapse of another year the average death-rate may be fearfully high, just because an epidemic intervened. This uncertainty can only be corrected when the *numbers are very large* and the *time embraced by them is considerable.*

2. To draw correct conclusions from numbers, one must be sure of their accuracy. I do not for a moment mean to question the veracity of any of the reporters I may quote, but I do doubt greatly the value of their reports. For example, the average most relied upon, as it is the most favourable to those who advocate maternities versus hospitals, is the Royal Maternity Charity, London, when the death-rate is only 1 in 334; but when I find in the words of Dr. Barnes, that the midwife "is bound to visit every patient at least three times after delivery," my knowledge of human nature leads me to suspect that though the rules of the Charity takes this as the minimum, the practice of the nurse will take it as the maximum. Now I find that of Dr. Collins' 88 cases of puerperal, 18 commenced after three days; of Drs. Johnston and Sinclair's 41 cases, 12 commenced after three days. It is clear that even if the nurse fulfilled the rule, a number of patients quite sufficient to have altered the death-rate very much, may have suffered from puerperal fever alone, to say nothing of other diseases which do not set in within three days. Further, if this mortality be trustworthy, then all one can say is that the poor of London are marvellously better off than the rich, for no private practice I have quoted shows such favourable statistics.

I say then that, not only must the reporter be trustworthy (and I do not question that), but he must be in circumstances to secure reliable statistics. This is just the difference between the data furnished by maternities and those from hospitals.

3. In comparing numbers, we must take care that we are comparing like with like.

If I understand the object of Dr. E. Kennedy's paper, it is to prove that the excessive mortality of large lying-in hospitals, as compared with small ones or with maternities, arises mainly from the prevalence of metria or puerperal fever.

But unless I mistake, Dr. E. Kennedy has taken the entire death-rate of the hospitals, and not the death-rate from puerperal fever, which makes a fearful difference.

I have not had time to hunt up the records of the foreign hospitals, but of our own I find in Dr. Collins' report, that the death-rate from puerperal fever was 1 in 297⅓; in Dr. M'Clintock's, 1 in 214; in Drs. Johnston and Sinclair, 1 in 289½; in Dr. E. Kennedy's, 1 in 112; whilst in private practice it will make an equal difference. Dr. E.

Kennedy gives 1 in 1,200; Dr. M. Clarke, 1 in 641; and my own, 1 in 424.

For the purpose of elucidating this difficult question, I have constructed three tables, showing the total number of cases, total number of deaths, and, where I could obtain it, the number of deaths from puerperal fever, and though, in my judgment, the numbers are far too small for reliable inferences, at least they will show the difference between the death-rate in small hospitals when taken separately or when added together. Having written so far before adding up the cases, I confess I was as much surprised at the result as any one can be. I can only say that as my object has been simply to find out the truth, so I am prepared to admit it, whether it contradicts my previous opinion (as in the present case) or confirms it.

1. Take first the table of maternities to which, for reasons I have already stated, I attach no value beyond showing that large numbers modify results seriously. In 236,665 cases there were 1,590 deaths, or about 1 in 149—a striking contrast to the death-rate of the Royal Maternity, although that report is included.

As a parallel has been drawn between the results of surgical operations in hospitals and at home, and the delivery of patients in hospitals and at home, I may be permitted to deny the analogy altogether. When a surgeon performs a "limb amputation" at the patient's house, he is quite sure to see his patient every day till he is cured or carried off, and if he tells truth, his statistics will be as reliable as the statistics from any hospital, whereas we have no evidence of continued attendance upon patients delivered at their own houses.

2. Looking over the table of the small lying-in hospitals, I find a wide range of death-rate, from 1 in 226 at Waterford, to 1 in 31 in the York-road Hospital, and when the entire cases, 27,300, are divided by the number of deaths, 405, the death-rate is 1 in 67. In 4 hospitals I find the death-rate respectively from puerperal fever 1 in 107½; 1 in 214; 1 in 389; and 1 in 681.

3. In the large hospitals the mortality ranges from 1 in 13½, in the Maison d'Accouchement, Paris, to 1 in 177 in Johnston and Sinclair's report; and if we add the whole number together we find 71,091 cases and 1,156 deaths, or about 1 in 62. The total death-rate of the Rotunda is 1 in 82.

The death-rate from puerperal fever given by Dr. Collins is 1 in 297; by Dr. M'Clintock, 1 in 214; by Dr. Johnston, 1 in 289, and by Dr. E. Kennedy, 1 in 112.

Now, sir, if, as I hope and believe these statistics are accurately given, however imperfect they may be as to numbers, I think they at least establish this one practical point, viz:—that until we can find a much smaller death-rate among the small hospitals, we should be very cautious in destroying the large ones, or diverting them from their present use:—

Place	Cases	Deaths	Proportion	P. Fever	Deaths	Proportion
MATERNITIES.						
Wellesley Dispensary, .	442	4	1 in 100	about		
Royal Maternity — Dr. Barnes, . .	18,751	56	1 in 329			
St. Petersburgh, 18 years,	207,582	1,453	1 in 143			
Glasgow, 1857 to 1868, .	4,800	47	1 in 102			
Rotunda Hosp., 7 years,	617	10	1 in 61			
Coombe, . .	4,473	20	1 in 223			
	236,665	1,590	1 in 149	about		
LARGE HOSPITALS.						
Rotunda, Dr. J. Clarke, .	10,387	125	1 in 83			
Rotunda, Dr. Collins, .	16,654	164	1 in 101½	88	56	1 in 297
Rotunda, Drs. M'Clintock and Hardy, . .	6,634	65	1 in 102		31	1 in 214
Rotunda, Drs. Johnston and Sinclair, . .	11,874	67	1 in 177	119	41	1 in 289½
Rotunda, Dr. E. Kennedy	13,167	224	1 in 58		171	1 in 112
Maison d'Accouch, Paris, 1862, . . .	2,204	166	1 in 13¼			
St. Petersburgh, 15 years,	8,036	306	1 in 26⅔			
Madras, 1857 to 1859, .	2,135	39	1 in 54			
	71,091	1,156	1 in 62			
SMALL HOSPITALS.						
Dr. Beatty, 1834 to 1837,	1,182	17	1 in 69½		11	1 in 107½
Dr. Churchill, 1835 to 1839, . .	428	4	1 in 107		2	1 in 214
Montreal Hospital, .	1,949	17	1 in 114½		5	1 in 389
Waterford Hospital, .	3,409	15	1 in 227		5	1 in 681
Edinburgh Hospital, 1844 to 1868, . .	3,824	62	1 in 61			
Glasgow Hospital, 1857 to 1868, . .	3,673	62	1 in 59			

Place	Cases	Deaths	Proportion	P. Fever	Deaths	Proportion
SMALL HOSPITALS—*continued.*						
New Ross (Dr. E. Kennedy)	924	5	1 in 184			
London (York-road) Hospital, 24 years,	4,960	146	1 in 34	nearly		
British Lying-in Hospital, 17 years,	2,438	25	1 in 87			
Liverpool Hosp., 7 years,	1,092	11	1 in 99			
Melbourne Hospital, .	3,421	41	1 in 83½			
Limerick, . .			1 in 367			
	27,300	405	1 in 67		23	1 in 303 about

I have no doubt of the utility of maternities. They meet the wants of a large class of the poor to whom, for moral and social reasons, removal from their families would be a serious injury; and also of a class somewhat above the very poor. But for the latter, destitute of all comfort and food, and fresh air, and proper nursing, a hospital is surely a great and valuable refuge, unless, in addition to professional attendance, we could undertake to provide all other necessaries.

It may be a question whether, if we had no hospitals, it might not be more beneficial to the poor to provide small parochial hospitals rather than one large one, and for one special reason, viz.:—that in the event of puerperal fever making its appearance, the hospital can be closed with less inconvenience.

But we are not placed in such circumstances, and what might be right and proper in such a case, would, it appears to me, be both hasty and injudicious now.

Dr. Morgan mentioned that in the Lock Hospital they had two beds for lying-in cases in a small ward connected with the large wards by a door. Dr. Johnston had referred to the mental condition of patients attacked by puerperal fever, but in the small ward of which he spoke they had had 74 deliveries, and so far as he could obtain information only one death had occurred, though these patients were affected by venereal, deserted by their friends, and in that unhappy condition they were about to give birth to a child—and he mentioned the fact as an interesting one, that the patients had done very well in that ward, and that for the last twelve months there had been no case of puerperal fever, although there had been five or six of erysipelas in the ward adjoining, and into which the door opened.

Dr. Hewett observed that Dr. Kennedy must remember that of the cases to which he referred as occurring in the maternity district at large, there was a great number among the poorer class of which they had no direct method of getting at the exact truth. Dr. Kennedy would admit that isolation was not carried on among the poor. In the Coombe, when he was attending, it was a common thing to have several women at different stages, and some expecting to be delivered themselves. If there was any zymotic poison—whether it was of a fixed nature or not, which he did not think he had suggested—it was not communicated by the fluids of the parturient female, and therefore it must in some way be communicable through the air. Would Dr. Kennedy assert that in the different dwellings of the poor throughout Dublin this so-called zymotic poison would not spread to other parturient women in that house? and would they not have some observation of a number of women being attacked under these circumstances? It was well known to every dispensary practitioner that typhus fever would attack every room in a house, and that scarlatina would get into some of the rooms, and then spread to the whole of the children. He did not think there was any isolation amongst the poor, and that did away with the value of the statistics of external practice. If puerperal fever came under the genus zymotic disease, they should have the same occurring in other hospitals where zymotic diseases were treated. In the large hospitals in the poorer districts in London disease had not spread to the houses adjoining, although in one case the backs of them were within a few yards of the windows. It was a remarkable thing that in the case of zymotic disease they did not spread to the adjoining houses.

Dr. M'Clintock moved and Dr. Denham seconded the adjournment of the debate.

Dr. Kennedy thought he ought to be allowed to say a word in reply to what has been put forward; but, indeed, he had been left very little to do. Fortunately there had been such a discrepancy of opinion amongst the gentlemen who had spoken since his paper was read, that he thought they had answered each other pretty well. There were, however, a few points which required a little elucidation, and with these he would occupy their attention for a few moments. In the first place, Dr. Johnston mentioned that he said that there was a necessary recurrence of puerperal fever after any epidemic disease showed in the hospital. He (Dr. Kennedy) merely stated that it had been observed to occur. He used no such language as had been attributed to him. He had stated distinctly that it was of a capricious character, and if they wanted any proof, the opinion of the gentlemen who had spoken had left little doubt upon this. It was often quite impossible to arrive at its cause

any more than that of any other zymotic disease. If he asked Sir Dominic Corrigan to explain, on all occasions, why fever should appear, he would smile; and if he turned to any other gentleman there and asked him why a certain zymotic disease should appear, at a given time and place, he would be equally amused; and he, therefore, did not feel called upon to explain this. All he had done was to endeavour to throw some light upon it, by tracing, as far as he could, after an experience of 40 years, certain predispositions to it. One gentleman said he ought to consider it a little longer before forming an opinion. Why he had been considering it since before he was born—he had been 40 years thinking of it. Many an anxious night it had cost him, when he had been obliged to visit his patients every six hours, and from that time till this he had never lost sight of it. To ask him at this period of his life to sit down to consider it a little longer was, he thought, a little unreasonable (laughter). The gentleman who next addressed the meeting spoke of sporadic puerperal fever, and expected him to explain the whole phenomena of self-poisoning. He thought he went very near it, as near it as man could go, without having the thing before him, but he could not explain the whole phenomena. He got a schooling from another gentleman because he did not allude to the effect of examinations in producing a disease that was contagious. Well, he thought it did not require a philosopher to explain to this gentleman that the disease that he specified as contagious would be contagious to the patient if applied by the finger. He thought the question recoiled a little upon the speaker himself. This same caustic gentleman had dealt very strongly with him upon another point. He had his own little book there, and in order that he might not misrepresent him, although he mended his hand since he wrote it, he would give the quotation:—

"I cannot admit that the mere accident of a number of lying-in women under the same roof of itself gives origin to puerperal fever, though when once it appears under such circumstances it spreads. The deaths bear no relation to the number of women admitted."—*Extern Maternities*, by Loombe Atthill, M.D.

Then he instances Dr. Labatt's celebrated period, commencing in 1815; omits all mention of the opening the auxiliary hospital, and boasts of this period being conclusive on the point that deaths bear no relation to the number of patients admitted. Now let us look at the tables. In the year 1816 the deliveries were 3,276 with 1 death in 182. In 1817 the deliveries were increased by 200; the death-rate rose to 1 in 108; the deliveries were increased from 3,473 to 3,539. In 1818 the death-rate rose to 1 in 63, and although the deliveries were reduced next year to 3,197, the habitat was confirmed.

And what occurred after that? Although they lowered the

admissions to 3,100 next year, there was 1 in 33. What he wished to convey in his paper was, that this mysterious disease had been on the increase, and particularly for the last fifteen or twenty years, especially for the last fifteen, and this was what drew him out at this moment, and what made him urge upon his enlightened, intelligent brother professional men the necessity for meeting the urgency of the case. For fifteen years the death-rate had never fallen lower than 1 in 31½, and in one year it was so high as 1 in 14. Was it not time to raise his voice before he was swept away from amongst them? He had got very little more time in which to do it, but he had got upon the housetops. He did not mean to say that the number of patients was as great in the Lying-in Hospital; but he meant to say that the disease had now got firm hold, and that it would not be removed unless some steps were taken, and he knew of none more effective than the one he had suggested. He had only suggested what statistics and his own observation proved, and he wished to draw the attention, first of the governors of this hospital, and secondly, of the profession, when he failed with the governors. They were all very well aware of the efficacy of statistics at that moment, and of the rage there was for collecting them. He had been obliged to use them, though he did not rely upon any that he had not a perfect knowledge of. His calculations were never arrived at from the statistics he collected, but from observation—the result of close, protracted observation, extending over years, and growing with his growth and age. There was a marked difference between observation and statistics. Observation was the result of facts observed by a man himself, of which he had an inherent cognizance that could not be displaced from his mind. What were statistics? Talyrand said language was made to disguise ideas, and he might have extended the remark to figures. He might have said "figures are made to disguise quantities," and with perfect justice. What was a figure? It was a symbol of quantity. There was nothing in a figure more than in a word. One could tell lies with figures as well as with words, and bolder ones. It was very important to know this and to bear it in mind. He thought there were three kinds of statistics. Some of those present might recollect in reading those clever writings of Dickens' that one of his heroes, when he is asked if he likes sausages, answers, "Like sausages?—that depends very much if I knows the girl that makes them" (laughter). That was his feeling about statistics. His value of statistics depended very much on whether he knew who made them. There was a friend of his who had arrived at certain conclusions from statistics, and he changed his mind, but not on the principle of the young man who got sausages from the girl he knew. There were also what he called kaleidoscope statistics. A gentleman put in his figures, there was a process of *reflection*, and he turned round the

instrument, and each time a new form was produced, and they were just worth what they found them. Then there was another form—the Babbagean. Babbage made an instrument, into which he could put a figure, and amongst a million it would come out at the end of 50 years at the right spot, when he wanted it. Statistics were valuable if they were honest, and fairly put forward, and reliable. One of the speakers had said that he had drawn a comparison between the small provincial hospitals and the mortality in the maternities. He never drew a comparison with any maternity, because he had no confidence in their statistics, and he purposely avoided drawing a comparison with anything he had not confidence in. He hoped anything he had left unanswered had been met by the gentlemen on the other side, for, in the words of an eminent judge, "I agree with the two doctors for the reasons assigned by the other two who differ from them (laughter)."

The meeting then adjourned.

The adjourned discussion was resumed on 15th May by Dr. M'Clintock, who said:—

Mr. President and Gentlemen,—The subject treated in the elaborate paper of Dr. Kennedy is, I consider, one of the highest moment, demanding of us the fullest and most careful consideration; and in making it the matter of a special discussion we are only following the examples set us by the French and the New York Academies of Medicine, and the Norwegian Medical Society. Closely connected with it are many grave questions of a medical, therapeutical, and social character, which all tend to invest it with supreme interest and importance.

In order to be as brief as possible I shall, without further preface, take up some of the points brought forward by the author, and offer a few remarks upon them.

That true puerperal fever is a zymotic disease can hardly now be called in question.

No author that I am acquainted with has argued so conclusively in favour of its zymotic character as Professor Fordyce Barker, on the occasion already referred to, when this subject was discussed in the New York Academy in the year 1857. But that there are cases of peritoneal or uterine inflammation succeeding delivery which do not possess this zymotic character, and are therefore essentially instances of a pure primary phlegmasia, is not only strictly consistent with analogy, but is confirmed by clinical observation; but there may sometimes be a difficulty in distinguishing to which of the two classes a case belongs. The particular phlegmasia most likely, I think, to produce symptoms resembling those of zymotic metria is uterine phlebitis; and yet, as Professor Barker says, "we may have phlebitis in the puerperal woman, and not have puerperal fever;" and my own experience corroborates this statement.

At the outset of his inquiry Dr. Kennedy asks, "are we to ascribe the different zymotic poisons to a common principle, modified by a variation in natural or physical circumstances or conditions?" Some of his remarks with reference to this question lead us to suppose he would give an affirmative answer to it. Let me quote his own words:—"I have remarked that when continued fever, typhus or erysipelas, were prevalent in the medical and surgical hospitals, puerperal fever appeared to prevail in the Lying-in Hospital, as well as in the city generally. The character of the fever varied also at different times, and occasionally appeared to be influenced in its nature by any prevalent epidemic. So frequently has metria shown itself afterwards that it is now no longer esteemed an accidental *post hoc*, but that they stand in relation of cause and effect. This fact alone goes a considerable way in confirming the idea of a common poisonous principle or miasm." This doctrine, as I understand it, would seem to be scarcely tenable. Does the poison of scarlatina ever directly produce any other zymotic disease but scarlatina? or do we ever find that zymotic metria communicates measles, or small-pox, or scarlatina, or typhus? No doubt these diseases are remarkably aggravated, and the danger of them vastly increased when they attack a puerperal woman; but, to quote the language of Dr. Copland, "these diseases always preserve their identity and their specific forms, and the power of perpetuating or extending themselves. Their characters are not lost in those of puerperal fever; and if, during their course, the phenomena, or the internal lesions and complications usually observed in malignant puerperal fever, appear also in them, they are merely superadded, and become the causes of the greater malignancy and more rapid progress of the malady to a fatal issue."

The appearance of any of these zymotics in a lying-in hospital is much to be dreaded, not alone because of their enormous danger to puerperal patients, but also from the peculiar susceptibility of such patients to the action of any zymotic infection. That puerperal fever is more apt to appear at times when typhus, scarlatina, and erysipelas are prevalent in the district or in the population at large, is a fact on which my own experience entirely coincides with that of Dr. Kennedy; but it may, I think, be accounted for without supposing these several poisons to have a common origin—at all events, the logic of facts does not compel us to adopt this conclusion, and in a matter that is surrounded by so much that is subtle and obscure, to doubt is the highest wisdom. There may be certain states of the atmosphere which powerfully influence the extension of zymotics, either by favouring the spread or transference of their respective contagia, or by disposing the constitution to their reception.

To this presumed law of the fixed or immutable nature of zymotic diseases there are two notable exceptions, viz., erysipelas and puerperal

fever, which two diseases are mutually convertible—the one into the other—of that there can remain no manner of doubt; and hence they must be supposed to have a common zymotic poison or contagion for their production. But after all, though it is convenient to distinguish erysipelas and zymotic metria as two different diseases, still there is little room to doubt that they are one and the same disease.

That puerperal fever partook of the nature of erysipelas was maintained, over a hundred years ago, by Pouteau, who regarded puerperal fever as "an epidemic erysipelatous inflammation of the peritoneum." The same opinion was strongly expressed by the late Dr. J. C. Douglas, of this city, whilst Mr. Simon lays it down as a demonstrated fact "that a surgeon going to a puerperal woman, from attendance on a case of erysipelas, may convey the specific contagion to the uterine surface of his new patient, and occasion her death by that form of puerperal fever, which, under a special name, is in fact but intra-abdominal erysipelas. Indeed in this respect erysipelas has a peculiar place among zymotic diseases."

The common nature, the identity, in fact, of zymotic metria and traumatic erysipelas, I therefore look upon as being established; and the recognition of this important pathological fact has materially assisted to elucidate the study and comprehension of puerperal fever, and to clear away much of the confusion and obscurity which so long prevailed with regard to it. Cruveilhier was, I believe, the first pathologist who asserted that a recently delivered woman "presented a faithful picture of one who has undergone a serious surgical operation." In some subordinate details he may have overstrained this analogy, still his description is wonderfully accurate; and it furnishes us with the real anatomical clue to understanding why the secondary evils of parturition should be so similar to those of surgical operations. Simpson has written upon this particular subject at some length, and has ably worked out "the analogy between puerperal and surgical fever," in all its bearings and details. Whilst on this point it may not be uninteresting to mention that the late Mr. William Dease, of this city, states in his "Observations on Midwifery," published eighty-six years ago, that he had been much struck by the close resemblance between the symptoms and the anatomical lesions belonging to puerperal fever, and those which occasionally followed upon great operations on the bladder or the perineum in the male subject. In this observation we perceive a recognition, faint though it be, of the analogy between puerperal and surgical fever.

I speak under correction when I say that two of the salient points which Dr. Kennedy endeavoured to establish in the second part of his paper are, first, the highly contagious nature of puerperal fever, and second, that this disease has its habitat, and is endemic in large lying-in hospitals. At the same time he admits that it does occur sporadically

in private houses, and that epidemic influence may, to a certain extent, favour its extension and development.

Now that true puerperal fever—zymotic metria—is a communicable disease is an opinion I have always held and acted upon. Writing on this point two and twenty years ago, Dr. Hardy and I said—"Notwithstanding all that has been said or written to the contrary, we think that the contagiousness—or, to use the more explicit term suggested by Dr. Christison, the communicability—of puerperal fever in all its forms is a fact established on the most irrefragable evidence, so much so that it would now be almost criminal for any practitioner to act on the opposite assumption."

I agree with Dr. Kennedy in thinking that puerperal fever is about as contagious as erysipelas, or ordinary continued fever, but no more. It is very desirable that this question should be settled and the truth made known, as there is a widely prevalent opinion that metria of every kind, whether zymotic or non-zymotic, is pre-eminently and obstinately contagious, and under this idea practitioners have sometimes incurred weighty censure and have been stung with much self-reproach, when, in truth, they were clear of all blame.

A great deal of the evidence which has been adduced by writers in support of contagion is open to the serious objection of proving too much. Take, for instance, the case of Dr. Rutter, of Philadelphia. Who can for a moment believe that the contagion of puerperal fever should cling to a medical man during a ten days' rustication at 35 miles from the scene of his practice, and after a course of bathing and shaving (even of his head!) and putting on an entirely new suit of wearing apparel? If such were the contagiousness of this disease, one or other of two consequences must ensue—either that puerperal fever would be permanently rife among our patients, or that every practitioner should, on the first occurrence of a case, entirely relinquish his obstetric practice for some weeks or months.

After twenty-five years' practice I can conscientiously aver that no single instance ever occurred where there were the remotest grounds for supposing that I carried contagion to a patient; but then I was always careful to use the ordinary precautions. Cases of puerperal fever have occurred in my private practice, but they were at long and irregular intervals. In the spring of 1855, when the disease was so frightfully prevalent in this city, and so fatal in the Lying-in Hospital, I attended five ladies in different parts of town, and not one of them had even a threatening of the disease. Now, had any of them got it I might certainly have been suspected of bringing it to her from the hospital (of which I was then master).

Again, I frequently have had occasion to remark, as others have done before, that the order or succession of puerperal cases in hospitals was most irregular and capricious—one case in this ward, another in that,

and oftentimes consecutive cases appearing in wards the most widely distant from each other.

Whilst admitting that zymotic metria is, in all respects, contagiousness included, on a parallel with erysipelas, I must, by parity of reasoning, reject the contagiousness of the non-zymotic local inflammations, whether uterine or peritoneal, incident to childbed. As this element in the question before us has received only a small share of attention, I ask your permission to read a passage from the writings of the late Dr. Abercrombie, of Edinburgh:—"I have not seen so much of this disease as can entitle me to offer a decided opinion from personal observation; but from what I have seen, and from all the information I have been able to collect, I have little doubt that women in the puerperal state are liable to two distinct forms of peritonitis, which, in the discussion on this subject, have probably not been sufficiently distinguished from each other. They are liable to the common acute peritonitis, presenting the usual symptoms, yielding in a large proportion of cases to the usual treatment, and exhibiting in the fatal cases the usual morbid appearances of extensive pseudo-membraneous deposition and adhesion.

"But they are likewise liable to another form of disease, in which the symptoms are more insidious, and are accompanied from an early period by great prostration of strength and fever of a typhoid character. . . . There is little doubt that it is a contagious disease, or that it is capable of being conveyed from one woman who is affected with it to another who is in the puerperal state. It appears as an epidemic at particular times, being very frequent and very fatal while it prevails, and erysipelas, or other affections of an erysipelatous character, have often been observed to be prevalent at the same time."—(Dis. of Stomach, p. 188.)

In cases where there was the highest presumptive reason for supposing that the attack of puerperal fever was due to contagion, I have sometimes felt a difficulty in believing this in consequence of the extreme rapidity with which the effect has followed upon the supposed cause—a rapidity almost unknown amongst other morbid poisons, when not introduced by direct inoculation. It is a well-established fact, that morbid, like other poisons, have a certain period of latency or incubation before their specific actions come into operation. "The virus of natural small-pock," writes Professor Aitkin, "lies dormant from sixteen to twenty days before it produces any constitutional disturbance; and a still further period elapses of three or four days before the specific eruption appears on the skin. The poison of *scarlatina* lies latent from seven to ten days after exposure to the contagion, that of measles from ten to fourteen, while the poison of paludal fever has been said to be dormant for a twelvemonth, and that of hydrophobia for a still longer time" (Vol. i., p. 203). "Erysipelas occasionally follows a few hours after exposure to the infection. Dr. Elliotson thinks five days elapsed in his

own case, and Dr. Watson has given three cases in which the interval was a week. It has been observed in hospitals that a fortnight has elapsed after its subsiding in one case and appearing in another in the same ward. It is probable, therefore," Dr. Aitkin concludes, "that the period varies from two to fourteen days" (Ibid., 363).

The instances of puerperal fever, however, are innumerable where, if contagion excited the disease, it did so within a very few hours—from six to twelve—after the communication of the poison. At the same time, I can readily understand how an epidemic constitution of the air, by powerfully disposing the body to the action of a zymotic poison, may cause this to take effect with unwonted and exceptional rapidity.

Though but a limited contagionist, still I hold very strongly the producibility of puerperal fever by inoculation; as, for instance, where the hands of the accoucheur or nurse retaining some necroscopic matter or septic poison of any kind, come in contact with the highly absorbing surfaces of the maternal canals. The experience of Dr. Semelweiss at the Vienna Lying-in Hospital is, no doubt, familiar to you all, and needs no further mention in this place.

I cannot help thinking that if students, while attending at lying-in hospitals, were precluded from dissecting, or from being dressers at surgical hospitals, it would help to lessen the frequency of puerperal fever. I also think it would be well if the medical officers of lying-in hospitals were to abstain altogether from taking any share in *post mortem* examinations. I very much blame myself for not having rigidly acted on this when I had medical charge of the Rotunda; but, unfortunately, the anxiety to secure a specimen for the class or the museum had undue weight with me. I also venture to surmise, with Dr. Kennedy's permission, that his zeal, too, in the pursuit of pathological knowledge—a zeal which is amply attested by the splendid museum he formed—may have helped to swell his hospital mortality. Now that I am in the enjoyment of the honour of being President of the Pathological Society, there is no need to hide my opinion, that the man who devotes himself to obstetrics should abjure the study of pathological anatomy.

I quite agree with all Dr. Kennedy has said in disapproval of lying-in wards being attached to general hospitals. Both theory and experience point out to us that such an arrangement is in every way open to the gravest objection.

I must also admit that, according to the statistics he has brought forward, and his mode of dealing with them, the author has made out a strong case against the larger lying-in hospitals. But, whilst I readily concede the greater mortality amongst women confined in these institutions, as compared with the aggregate of women confined in their own homes, still I am of opinion that this difference has been greatly exaggerated, that it is only in part attributable to hospital influence,

and that the soundness of some of Dr. Kennedy's propositions and statistics bearing on this point may fairly be called in question.

He lays it down as a corollary, "that the generation and absorption of the metria contagium is *in direct proportion* to the number of parturient women cohabiting in their lying-in, and that its habitat is the great lying-in hospitals." Let us test the truth of this proposition by an appeal to some simple facts.

1. In the years 1842, 1843, and 1844, the deliveries in the Rotunda Hospital amounted to 6,634, and 31 of these women died of puerperal fever, and of puerperal inflammation of one kind or another, being in the proportion of only 1 in 214.

2. In the years 1850, 1851, 1852, and 1853, out of 7,919 women confined in the hospital, 14 died of puerperal fever, or only 1 in 565.

3. Again. For four and a half years successively (from 1829 to 1833), in the same hospital, with the annual number of deliveries exceeding 2,150—double what they have latterly been—*not a single* death from puerperal fever occurred! That is to say, out of 10,785 patients delivered in the wards not one died from puerperal fever. This statement would, perhaps, be scarcely credited if it did not rest on the unimpeachable testimony of Dr. Collins.

4. During the last fifteen years the average number of patients in the Rotunda Hospital was very little more than one-half that of previous years, nevertheless the rate of mortality has been double as great.

5. Within the same period of fifteen years, so often referred to by Dr. Kennedy, the highest rate of annual mortality does not coincide with the years when the admissions were greatest, but quite the reverse.

6. Dr. Phelan states, that in the first three years of Dr. Labatt's mastership, the immense number of 10,248 deliveries took place in the hospital, and only 57 women, or 1 in 180 died. During these years, be it remembered, the annual number of deliveries was treble what it has been for several years back; in fact, it was a common thing for the patients to be lying two in a bed!

7. Lastly, to contradict the assertion that the rate of mortality is directly influenced by the size of the hospital, we may cite the New Ross Hospital, mentioned by Dr. Kennedy. This little country hospital had an average of 30 admissions per annum, not a third of the annual admissions to the Waterford Hospital; nevertheless, its mortality was, to quote Dr. Kennedy's own figures, 1 in 185, whilst that of the Waterford Hospital, with three times the annual number of deliveries, was only 1 in 227.

It is demonstrable, then, from the experience of these hospitals, that the presumed generation and absorption of the contagium of puerperal fever being in "a direct proportion to the number of parturient females" congregated together under the same roof, is incorrect, or must be

received with some qualification which the author has not attached to it. At the same time, I agree entirely with Dr. Kennedy, that when an epidemic of puerperal fever invades a lying-in hospital, the intensity as well as the extent of its ravages will be proportionate to the number of patients in hospital at the time. To my mind, one of the most unanswerable arguments in favour of the occasional origin of puerperal fever from epidemic influence is the appearance, and again the disappearance, of this disease in the same hospital, under the same system of management, and under the same medical officers. Dr. Kidd has forcibly dwelt on this argument, so that I need not enlarge upon it.

In his tenth proposition, Dr. Kennedy lays it down that zymotic metria "is not a disease observed to occur in small lying-in hospitals or cottages, where only one or two patients cohabit in their lying-in."

1. To refute this statement I should suppose it were enough to cite the instance of the Waterford Lying-in Hospital—with an average of only 115 deliveries annually—in which, nevertheless, there occurred five deaths from puerperal fever.

I must beg, however, to bring forward some further evidence on this point.

2. "The Liverpool Lying-in Hospital is a small but exceedingly clean and apparently well-conducted institution"—(Holmes.) The average number of annual deliveries is about 150. The gross mortality in the seven years ending with 1862, was one per cent. The causes of death are not specified excepting in the years 1858 and 1861; in the former of these years *two* deaths, and in the latter three deaths resulted from puerperal fever; and Mr. Holmes, from whose report I quote, pertinently adds, "It must not be forgotten, however, in considering the rate of mortality, that the patients admitted are expressly respectable married women."

3. The South-Eastern Lying-in Hospital received about 350 patients per annum, and may therefore be included among the small hospitals. I feel great satisfaction in appealing to the statistics of this little hospital, knowing that they are worthy of implicit confidence. Dr. Beatty, the founder and generous supporter of this hospital, thus prefaces his clinical report of its practice—"One of the most important services which can be rendered to the science of midwifery by an institution such as this, will be afforded by an accurately preserved register of cases treated within its walls. Such a register was carefully kept in the record of the South-Eastern Lying-in Hospital."

The entire number of women delivered in this hospital was 1,182; the death-rate among these was 1 in 69; and the number of deaths from puerperal fever and inflammation, was 12, or 1 in 98.

Whatever objection may be taken to the statistics of this hospital, on the score of its size, such, certainly, cannot apply to the institution I next refer to.

4. The Kingstown Lying-in Hospital, both in point of size and situation, ought surely, in every point of view, to be an unexceptionable specimen of a cottage hospital—the annual number of deliveries in it not exceeding 100. I believe it was closed in 1863; at all events during the seven years preceding this, there were 685 patients delivered in it, out of which number 10 women died, 6 of them, or 1 in 114, being from puerperal fever.

Although the foregoing instances have been cited for the purpose of proving that lying-in hospitals on the smallest scale are not exempt from puerperal fever, still the same might logically have been inferred from the admissions of the author, that the poison of zymotic metria "might be generated by any parturient female;" and that this disease "is to be met with in the hovels of the poor and the chambers of the wealthier classes;" which I consider is tantamount to saying that it may occur sporadically; and surely if it may occur in a private house where only one patient is confined in the year, why not in a cottage hospital where 50 to 100 are confined in the same space of time?

Here I find myself,—most reluctantly, I assure you,—drifting into statistics, a subject I very much dislike and very much distrust. For, although like the microscope it can reveal truth, yet like that valuable aid to knowledge it requires to be handled with caution, and its indications interpreted by one thoroughly trained and habituated to its use. I really believe there has been of late years no more abundant source of error in medicine, surgery, and hygiene, then the crude, narrow, or improper use of statistics. We have had some memorable illustrations of this in questions connected with practical midwifery, which I am sure will be fresh in your recollection. I do not, therefore, purpose examining into the accuracy of all the author's statistical calculations; and in fact I could not possibly have done so in the short period which has elapsed since his paper came into my hands.

Whilst admitting that the illustrations he and Mr. Phelan have brought forward of other obstetric institutions are in their way instructive, and have an approximative value, still, upon the great question before us, they cannot be looked upon as conclusive. I protest, too, against the death-rate of three small provincial lying-in charities, differing (1) in their geographical position, (2) in the periods over which the rates extend, and (3) in the social and physical condition of the patients resorting to them,—being taken as the true standard by which to measure the healthfulness of any large metropolitan lying-in hospital.

It is not to be supposed, for example, that an hospital in the centre of a crowded metropolis will be supplied with as healthy inmates as one in the country, or, it may be, in a provincial town. And further, the average mortality among lying-in women varies very considerably in different places. For example, Professor Faye, of Christiania, states that

during eight years there died among parturient women of the higher classes of that city, 1 in 35; whereas Dr. Clarke's practice here shows only 1 death in 175; Dr. Crosse, of Norwich, had 1 death in 98; and Dr. Mantell, of Lewes, 1 in 1,205. Again, there are periods of immunity from epidemic disease, and periods when such is prevalent, so that if a report does not extend over a sufficient length of time, as Dr. Churchill has remarked, it will not yield a fair average; and this observation holds good equally of towns and communities, as of hospitals.

Lastly, the physical, social, and other conditions of patients resorting to an hospital cannot but influence considerably the results of treatment. By the rules of some institutions none but "respectable married women" will be admitted; another will not receive women in their first labours; whilst a third, in the true spirit of Christian charity, requires no other claim to admission within its walls than the pangs of childbirth.

There is no class on whom the dangers of childbed fall so heavily as outcast or seduced women, and even where a choice is open to them, such patients will of preference resort to a *large* hospital, where they are less likely to be known, or their private history found out. Of the women confined in the Rotunda Hospital, during my mastership, *one hundred and twenty-seven* were known to be unmarried; and *thirty-one*, or very nearly one-fourth of the entire number, died.

The question of death-rates is undeniably one of such vast importance—whatever way we may view it—that I am tempted to borrow from the writings of Mr. Simon, one of the highest authorities on such a matter. "Of all the various forms in which medical or surgical unsuccess may become evident, DEATH is the most unquestionable, and if proper materials existed for the purpose, hospitals might well be compared with each other, in regard of their respective quantities of that particular sort of unsuccess. But in order that such comparisons may be something better than mere arithmetical idleness, it is essential that there be no error as to the principle on which the death-rates are to be estimated. Death-rates calculated on *given magnitudes of illness* (*i.e.*, the compound result of number and quality of cases) are, as far as they go, true measures of medical or surgical unsuccess. But a definite magnitude of illness is not necessarily represented by any given number of patients taken indiscriminately. Magnitude of illness, as regards the number of deaths which are likely to result, may be almost infinitely greater in one set of patients than in another equally numerous set. . . . . During the present grievous imperfectness of hospital statistics, we cannot by such statistics accurately compare even in part (much less in entirety) the success of one hospital with the success of other hospitals, nor *à fortiori* the healthiness of one hospital with the healthiness of others."[a] So far Mr. Simon. Now

[a] Sixth Report of the Medical Officer of the Privy Council, pages 40 and 51.

permit me to give an extract from the report of Drs. Bristow and Holmes:—

"We do not hesitate to draw from the condition of the death-rate of an hospital a very different inference from that which it is fashionable to draw. We do not infer that a high death-rate is a sign of healthiness (for this, of course, we have no warrant); but we *do* draw this inference, and we are satisfied of its foundation, in fact, that a high death-rate indicates, as a rule, that an hospital fulfils efficiently the purposes for which it was designed, and that a low death-rate, on the other hand, indicates (*cæteris paribus*) comparative inefficiency."[a]

The principle enunciated in these quotations applies with little qualification to maternity as well as surgical and medical hospitals.

A reliable estimate of the mortality among lying-in women confined at their own homes is a very great desideratum. I must honestly declare my conviction that up to the present time, notwithstanding our multiplied and elaborate Registration Reports, there is no reliable return of such deaths, and therefore it is no better than "arithmetical idleness" to be constructing out of these reports any standard of comparison between hospital and home midwifery practice: and this opinion is shared in by every medical man of experience who has bestowed any consideration on this matter. Nor have we to go far to discover the reason of this. The death of a woman in childbed, as every one here well knows, always attracts a great deal of attention, and is a fertile subject for popular comment and animadversion; but, if the cause of death is known to be puerperal fever—or anything pertaining thereto,—then, indeed, quite a panic is created in the neighbourhood, and both doctor and nurse come in for more than their full share of blame. Hence, for their own reputation's sake, as well with the charitable motive of not alarming all the pregnant women in the community, the death is imputed to any other possible cause rather than to the dreaded puerperal.

It is not necessary, however, that any motive be assigned for this. The defect lies in our system of registration—not in those who supply the returns. Practitioners make a very proper distinction between dying *in* childbirth, and dying *of* childbirth. When a woman happens to die in childbed of some intercurrent disease—as phthisis, pneumonia, dysentery, apoplexy, albuminuria, bronchitis, morbus cordis, &c—this alone is returned—and rightly so—to the registrar as the cause of death. *Consequently all these deaths have no place in the registration reports of deaths in childbed. But a lying-in hospital is debited with every death occurring among its patients*—whether the death arise directly from parturition, from puerperal disease, or from any accidental intercurrent disease.

In this way, I think, we can account for much of the discrepancy

[a] Sixth Report of the Medical Officer of the Privy Council, p. 529.

between the death-rate of lying-in hospitals and that deduced from the returns of the Registrar-General. To illustrate my meaning and to show how statistics must be influenced by this source of inaccuracy, I have compiled the following table:—

TABLE I.—*Showing Deaths in Childbed, classified under three heads. (From Home Practice exclusively.)*

AUTHORITY	Number of Cases	Deaths from Accidents of Labour	Deaths from Puerperal Diseases	Deaths from non-Puerperal Diseases	TOTALS
Joseph Clarke, . .	3,847	7	6	9	22
J. G. Crosse, . .	1,377	1	8	5	14
John Beatty, . .	5,616	2	9	2	13
Thomas E. Beatty, .	2,064	4	8	5	17
Churchill, . . .	2,548	6	8	2	16
Browne,[a] . . .	—	22	6	7	35
M'Clintock, . .	652	0	4	2	6
Sir P. Dun's Maternity,	336	—	—	2	2
Brunker, . . .	334	3	3	0	6
TOTALS,	16,774	45	52	34	131

In compiling this table the results of private practice chiefly are taken, as being *thoroughly reliable*, and the patients having been all attended at home. Hospital statistics are excluded, because the great excess of metria cases in hospitals would necessarily disqualify them from showing the proportion that deaths from non-puerperal disease bear to all other deaths in childbed among patients out of hospital.

From this table it is apparent that the deaths occurring in childbed from non-puerperal disease form considerably over one-fourth of all the fatalities. You can at once perceive the great effect which the omission of these deaths must have in lowering the death-rate of childbirth out of hospital. For example, when the deaths are returned as 1 in 200, they should, on the principle just laid down, be 1 in 150. Or, one other example; 11,722 English women died in childbirth in the years 1838, 1839, 1840, and 1841—the rate of mortality being one death to 171 births, or 168 women (after making the allowance for twins).[b]

[a] Dr. John Brown, of Dundalk. The above results are from twenty years' practice, private and dispensary.

[b] Fifth Annual Report of the Registrar-General for England and Wales.

But if what I have stated be correct—viz., that only three-fourths of the gross number of deaths are registered, the true rate of mortality is found to be *one* in 126.[a]

The foregoing observations, if wholly unsupported by collateral evidence, would perhaps carry little weight upon the great question which now engages our attention; but fortunately we are not without some data of indisputable accuracy on which we may found the rate of mortality in home midwifery, and these, so far as they go, tend to corroborate the view I have expressed.

1. Dr. Willan in his "Report on the Diseases of London," states that of 2,982 ladies delivered in that city under the care of an eminent accoucheur, 30 died, or 1 in 99.

2. Dr. Crosse of Norwich, exclusive of consultation cases, attended 1,377 ladies in their confinements, and 14, or 1 in 98, of these died.

3. The late Dr. Joseph Clarke of this city, attended in the course of 44 years' practice 3,847 patients, 22 of whom died, being in the proportion of 1 in 174.

4. The late Dr. Labatt, in his fifty years' practice, attended 4,368 labours, and lost 26 patients, being 1 death in 168.

5. In private practice I have had 652 cases, of whom 6 died, being at the rate of 1 in 108.

6. Dr. Brunker, Surgeon to the Co. Louth Infirmary, a man of well-known reputation and extensive practice in his own and the adjoining counties, tells me he has attended in private practice 334 midwifery cases, and lost 6 patients.

7. Dr. Churchill has attended 2,548 patients, and had 16 deaths.

If we add all these together, the sum is 16,108—and if into it we divide the 120 deaths, the result brought out is, 1 death in 134 cases.

It must be owned that one objection of a serious kind attaches to these statistics, viz., they are derived entirely from the middle and upper ranks of society. But, if among women in comfortable or luxurious homes—with every want supplied, and lacking no care that medical skill could supply or money command,—if, I say, among this favoured class, the mortality in childbed amounts to 1 in 134—what must it be among the lowest class of urban society, in the wretched abodes of poverty, with all its attendants of filth, impurity, and want?

I have just now deduced a death-rate of 1 in 134 from returns which are above suspicion. To the Registrar-General's death-rate, in childbed, for all England and Wales I have applied the principle already laid down, of increasing the deaths by one-fourth the entire number, and the result

[a] In justice to the returns of the Registrar-General, it must here be stated that they do not profess to include *all* the women dying in childbed, but only those dying from the effects or accidents of labour, and from puerperal diseases, which are included under the comprehensive term *metria*.

arrived at was 1 death in 126, which is certainly very close to the above rate (1 in 134), and so far corroborates the calculations on which that was founded. Let us test this by another process. In the Dublin Registration district, the deaths in childbed, from all causes, during the years 1864, 5, 6, 7, and 8, were returned at 373; substracting from this the 206 deaths in the Coombe and Rotunda during same period, we find the rate of mortality among women confined out of hospitals, in this city and suburbs, during the last five years, to be 1 in 207. Now if—as I believe we are justified in doing—we increase this death-rate by the deaths in childbed from non-puerperal diseases (and which I have shown amount to over one-fourth of all the deaths in childbed), this Dublin death-rate rises from 1 in 207 to 1 in 156, which approximates to the results obtained by two other and totally independent series of statistical calculations, there is yet another way of proving the truth of what has been advanced.

But here I must pause a moment to direct attention to one of Dr. Kennedy's processes of statistical computation. At page 289 of his paper he enters upon a series of obscure calculations, from all which he draws this very startling conclusion, "that in all the deaths that have occurred in Dublin for the last seven years in parturition, out of every 9 deaths 7½ women have died, who would in all human probability be at this moment alive had they been confined in their own homes or in isolated cottage hospitals."

I think it might be shown that this chain of reasoning is utterly fallacious; but I am not going to enter into that, inasmuch as a great numerical error, represented by the number 8,688, lies at the very basis of his calculations, and vitiates all the conclusions derived from them. The error is this. He gives the total number of births in the Dublin district, for the period specified, as 52,126, whereas it was only 43,438.

Let me now return from this digression. There is yet another way by which it may be shown, if not demonstrated—that one-fourth is not at all too much to allow for the deaths omitted in the registration returns of deaths in childbed. By looking at Table I. you see that the proportion of deaths from the accidents of labour, to those from non-puerperal diseases, is 45 to 34. Table II. shows the relation between these classes of deaths in hospital practice, and here the relation is very much closer, so that were I to include them, it would greatly strengthen my position; but I confine myself to Table I. as being based on home practice. Now applying this ratio to the Dublin statistics of *home* midwifery, in which the deaths from accidents of labour were 126, we have a simple question in proportion, viz.:—45 : 34 :: 126 : to a fourth proportional, which by a very simple calculation is found to be 95.

This process shows the deaths from non-puerperal diseases to be greater than one-fourth of the entire number of deaths, which coincides with what I estimated it by my former calculations. Adding this 95 to the gross

number of deaths reported—167—we get as a result 262, and dividing this into the total number of births returned *out of* hospital—34,590—(after making allowance for twins), we get as a quotient 131, whereby we find that the death-rate among women confined at their homes, in and around Dublin, in the last five years, was 1 in 132.[a]

TABLE II.—*Showing the Number of Deaths in Childbed from the Accidents of Labour and from Non-Puerperal Diseases respectively, among* 71,378 *Women, in Hospital Practice.*

HOSPITAL AND AUTHORITY	DEATHS		TOTALS
	From the Accidents of Labour	From non-Puerperal Diseases	
Rotunda—J. Clarke, ...	16	41	57
Do. Collins, ...	62	38	100
Do. M'Clintock and Hardy, . .	22	11	33
Do. Johnston and Sinclair, . .	53	40	93
Do. M'Clintock, ...	36	45	81
Do. Madden, ...	4	7	11
Coombe—Kidd, ...	19	12	31
British—G. Hewitt, ...	—	7	7
South Eastern—Beatty, . . .	2	1	3
Waterford—Elliott, ...	8	2	10
TOTALS, . .	222	204	426

I have taken up a great deal of your time in proving that the public registration of deaths only gives at most three-fourths of all the deaths in childbed.

I am sorry to say I am not quite done with statistics. At page 304 of his paper, Dr. Kennedy deduces from the returns of three small hospitals, a death-rate of 1 in 282⅔, which he sets up as his "standard of the proportion of deaths which occur where women in labour are treated in small hospitals in place of large." With this as a starting point, and ignoring any other difference between small and large

[a] In connexion with the above mortality rate it is worth observing, that the obstetric mortality of Norway, for the five years, 1859–63, according to the report of Professor Faye and Hr. Schönberg, was in the proportion of 1 death in 135 confinements. (British and Foreign Medical Review, No. lxxxv).

hospitals than "the congregating large numbers of parturients into the same building in one case and not in another"—he forthwith enumerates ten different lying-in hospitals, and points out the proportion of women in each that died, but "should not" have died—this proportion ranging from 3 out of 4 to 20 out of 21.

Here again Dr. Kennedy has committed a serious arithmetical mistake in his data, which consequently falsifies all the conclusions he has laboured to draw from them. He takes the mortality of the Waterford Hospital at 1 in 295, whereas it was 1 in 227;[a] so that the mean rate of mortality in these three small hospitals is 1 in 259, and not 1 in 282⅔, as stated by him. Dr. Churchill gave us a table to supply information on this very point—viz., the rate of mortality in small hospitals, and he lays it down at 1 in 65, from the statistics of 11 hospitals; and I don't see that Dr. Kennedy, in his reply, refuted this inference.

But setting this aside, I ask any man in this room whether such limited data—the returns of three very small country hospitals—are sufficient to form an infallible standard of comparison, and to justify the heavy accusation Dr. Kennedy brings against ten great obstetric institutions? or whether it is correct to say that the *only difference* between the two classes of hospitals is that derived from the number of their patients?

Surely the physical quality of the patients, the general description of cases, the proportion of dangerous and complicated cases, differ as widely as possible in every circumstance and particular, capable of affecting the rate of mortality.

But, sir, in such an assembly it would be unnecessary to enter minutely upon this particular branch of the subject. There are many circumstances, however, in the case of large lying-in hospitals which concur to render their death-rate necessarily very much higher than that among patients confined at home; and *without making due allowance for all these circumstances, any comparison between the results of home and hospital practice, is absurd and unjust.* Let me here briefly enumerate some of the circumstances which contribute in no small degree to swell the mortality of a great lying-in hospital:—

1. The disproportionately large number of primiparous women who are confined there. The only data I can find to show the proportion which primiparæ bear to all other parturients are those derived from the Scotch Registration Reports (as quoted by Dr. Matthews Duncan). From these it would appear that about 23 per cent. of parturients are primiparous. But during Collins's mastership of the Rotunda

[a] In fixing the death-rate of the Waterford Hospital at 1 in 227, I take the *whole period* (29½ years) over which Dr. Elliott's report extends, as stated by Dr. Kennedy himself at p. 302 of his essay. During this period the number of women delivered in the hospital was 3,409, the deaths were 15, and the number of deliveries 3,458. (See Dr. Elliott's report in the number of this journal for November, 1867).

(1826-33) the primiparæ numbered 30½ per cent., and during my own mastership (1854-61) the primiparæ were 37 per cent. of the entire number delivered. Now, Dr. M. Duncan has investigated with his usual care and ability "the relation of the number of the labour to the mortality accompanying parturition," and the results at which he arrives are as follows:—

"The mortality of first labours is about twice the mortality of all subsequent labours taken together." "The mortality from puerperal fever following first labours is about twice the mortality from puerperal fever following all subsequent labours taken together."[a]

2. I have already alluded to the number of seduced or deserted women who seek the refuge and concealment of a large hospital. At the time of their lying-in every one knows how peculiarly obnoxious these patients are to the fatal influences of labour and childbed. During my seven years' mastership of the Rotunda (1854 to 1861) it came to my knowledge, without making any special inquiries regarding it, that 127 patients were unmarried women, and had come from every part of the country, and of these 31, or very nearly one-fourth, died in childbed, and chiefly from some form of metria.

3. A large lying-in hospital of any established character will be sure to attract, even from distant places, cases of a rare, difficult, or complicated kind (*e.g.*, pelvic distortion, pelvic, ovarian, or uterine tumours, vaginal cicatrices, pregnancy complicated with organic disease, &c., &c.). This is a consequence of the hospital's reputation, and I for one do not complain of it; but the influx of such cases must inevitably add to the mortality of the institution.

4. Many deaths occur in hospital of patients who, on admission, are in a dying state. Surely it would be unfair to charge the hospital with such fatalities, and yet they are included in its death-rate. To show that these cases form no inconsiderable item, I may just mention, that of the 163 patients that died in hospital during the mastership of Dr. Shekleton (1847 to 1854), *seventeen* were actually in a hopeless or all but dying state, at the time of their admission, from hemorrhage, ruptured uterus, convulsions, prolonged labour, &c. And during my own mastership of the same hospital the number of women admitted under precisely the same circumstances was *twenty-three*.

5. It is no uncommon thing for patients to come into hospital in labour who at the time are the subjects of disease, or have the germs of disease in their system. In some cases I have known scarlatina show itself a few hours after delivery, or even during labour. Unequivocal symptoms of puerperal fever have appeared occasionally in patients before delivery, or even before admission into hospital.

[a] Fecundity, Fertility, and Sterility, p. 247.

Dr. Denham said—

After the exhaustive addresses we have heard in reply to the subject now under discussion, I feel considerable reluctance in rising to occupy the time of the Society, because I have, to a very large extent, been anticipated in the remarks I intended to have made. Silence upon my part, however, might be construed into acquiescence in the principles and practice advocated in this remarkable paper; and, therefore, do I venture to ask your indulgence for a very brief space of time. And first, I should beg leave to read a sentence from Dr. E. K.'s paper:—

"I know no more harrassing trial to the consultant, in these cases, than to have been called into and assisted in subduing, the acute symptoms by prompt and energetic treatment, and then, when matters have improved, to be relieved from attendance under the impression that the danger was over, only to be recalled to see the patient, perhaps, forty-eight hours later, beyond the reach of treatment, from the inflammation having been again allowed to creep on to an impending fatal issue. This has occurred so frequently in my own case, that it left me in doubt whether, for my peace of mind, I should not refuse altogether to see such cases in consultation."

This paragraph speaks volumes for the practical skill and tender-heartedness of the author, but pays a very doubtful compliment to the midwifery practitioners of Dublin. I stop not, however, to comment either on the ignorance or culpable neglect of the physicians in attendance, who allowed the inflammation *again* to make head after it had been promptly subdued by the consultant when first called in, or to the indifference to pecuniary emolument, so nobly shown by him *in almost refusing* to see such cases, or meet such practitioners in consultation. What I want to show is that it gives us a link in the chain of argument in the present discussion; and it is this—If Dr. E. K. was called into such cases so frequently, and if his contemporaries—such men as Collins and Johnston, and Montgomery, and Beatty, and Churchill, with many others equally talented, and enjoying to as large an extent the confidence both of the profession and the public—if they were called in as often as he was under similar circumstances, I think we have a very strong argument to show that the disease prevailed from time to time, even among the rich, to a very large extent, and had a habitat outside as well as within the walls of our hospitals.

The statistics of out-door lying-in patients I pronounce to be utterly worthless, and unreliable. Indeed Dr. E. Kennedy, in his facetious reply at the close of the last evening, admitted as much—nay more, he must have known it when he penned his elaborate papers, for the fact has long been patent as the noon-day sun to all who have paid any attention to the subject, and yet he enters into the most elaborate calculations,

occupying whole pages of figures, beginning with his own 1 in 1,200 and ending with those of his friend, Mr. Phelan, whose calculations have been so unmercifully proved to be worse than useless by their manifold and palpable mistakes. And for what purpose? all to prove the murderous death-rates in lying-in hospitals and maternities as compared with out-door midwifery practice. Permit me to imagine for one moment, that Dr. E. Kennedy's practice had been amongst the poor of our city, and that his 3,500 patients had lived in the wretched garrets and cellars from which most of our hospital patients come, and that he had attended them in their squalid filth and naked poverty—many of them already diseased, and also too many of them the victims of seduction—would he have been able to write out his death-rate at 1 in 1,200. I venture to affirm that it never would have graced the pages of the journal, but would have been buried in oblivion, with many of his hapless patients. To any who may not know the fact, I am prepared to prove that many, very many, poor women attended at their homes by students or midwives, are never seen more than once or twice after their delivery, and that they are often, very often, obliged to seek advice from the dispensary doctor of the district, or go into some neighbouring hospital. Under such circumstances those patients are seldom put down as puerperal cases, but whether they live or die they never again trouble the registry of the maternity or hospital from which, in the first instance, they received assistance, but remain there *natural labours recovered*, as first entered, to swell the list of successful out-door practice, and proclaim aloud the super-excellence of home relief over that of hospitals.

The subject of small hospitals and maternities has been so ably handled by Dr. Churchill, that I shall pass it over with a single remark, and it is this—that you cannot draw any just comparison between a large hospital in a city like this and a small one in a country town. To bring the matter fairly to the test, you must have small hospitals in large cities, and you must see that the patients in both are all similarly circumstanced. No such experiment has ever yet been made.

The second proposition laid down by Dr. E. Kennedy, namely, that the poison generated by the parturient female may be reabsorbed into the system, and self-contamination thus be produced applies with equal force to patients at their homes as well as those in hospital; it is, however, a mere assumption, and we must have some proof of its existence before we admit it as an argument. This converting of parturient women into hermaphrodites is by no means an original idea of Dr. E. Kennedy, as we find Dr. Semelweiss of Pesth, in his work on the *Etiology, Nature, and Prophylaxis of Puerperal Fever*, stating that in some rare cases the decomposed matter is generated within the limits of the parts affected, and these are cases of self-infection. Professor Simpson does not go quite so far; he states, the disease may be propagated by the morbid effusions of

one puerperal woman when inoculated into another. He goes on to say, in such cases two or three days after delivery usually elapse before the disease breaks out, or, in other words, there is a latent period like what we see in small-pox and other such diseases, and this is the point to which I now wish particularly to direct the attention of the Society. When we vaccinate a child, or where an individual is exposed to the poison of scarlatina or small-pox, we all know that there is a period of incubation before any constitutional symptoms develop themselves. Generally speaking, the discharge from a parturient woman is purely sanguineous for hours, or even days after delivery, and the morbid effusions spoken of as liable to be reabsorbed, do not appear till the second or third day.

If reabsorbed, as has been asserted, where does the process take place, and what is the length of the period of incubation? I have seen women shiver 14, 16, 17, and 20 hours after delivery. When was the poison absorbed in those cases, and what was the period of incubation? I answer, before they ever entered the hospital at all. I have seen symptoms of the disease developed before delivery, and so has Dr. E. K. The case of Ann Brennan, so graphically described in the paper now under discussion, is quite to the point. She was admitted into hospital at 11 o'clock a.m.; at 2 the pupil on duty reported that she complained of great pain in her right thigh. She states that about a week before she had had a rigor, and that yesterday she had another shivering fit. Twelve leeches were applied, and within an hour she expressed herself much relieved; in two hours more she was dead, undelivered. The *post mortem* gave undoubted evidence of puerperal disease. The local depletion, we are told, gave her much relief; but whether or not it was supposed to hasten her death is not mentioned. Dr. Semelweiss gives a similar case, in which he also performed the Cæsarean section; and although so rabidly in favour of infection being conveyed by the fingers of nurses and medical students, admits having seen twenty cases of purpueral in women before delivery. Immediately after the epidemic of 1861-2, I visited the hospitals—Paris, Munich, and Vienna—and found they had all suffered severely at the same time that we did. Dr. Braun, of Vienna, had told me he had seen thirty-six women attacked with the disease during that winter, before delivery. On asking him if he thought the disease was induced or kept up by the students, his reply was short, but expressive—"We have the students always with us, the puerperal only sometimes."

The statement put forward by Dr. Kidd in his very able and practical remarks, on the first evening of this discussion, respecting the hospital in Vienna, is perfectly correct.

Dr. Braun informs me that the theory put forward by Dr. Semelweiss had been entirely upset during the last outbreak of puerperal. The midwifery hospital in Vienna, as you are all aware, is divided into two

departments—one being devoted to the instruction of medical students, while the other is entirely managed by nurse-tenders. It so happened that during Dr. Semelweiss' residence in Vienna puerperal fever was much more prevalent and fatal in the department allotted to the students than in the other; and upon this circumstance he founded his theory and supposed discovery of prevention by washing the hands of the students with chlorine. Unfortunately for this view of the case, during last winter the disease was more frequent and more fatal in the female department than in that to which the medical students had access.

The professor at Munich mentioned to me a very interesting fact—namely, that their hospital, a very large and elegant building, had been opened two years before for the reception of patients, with a new house, new beds, new blankets, and new sheets, and also with a new staff of nurses, yet puerperal broke out at the very first take-in of patients.

The fallacy of the third proposition, namely, that the generation and absorption of this contagion is in direct proportion to the number of parturient women cohabiting at the parturient period has been so fully and clearly shown that I need scarcely allude to it. I cannot, however, resist giving one or two examples from the reports of the Rotunda Hospital to show how entirely contrary to facts this proposition is. Thus in 1813 the deliveries were 2,484; the deaths were 62, or 1 in 40. In 1825 the deliveries were 2,746; the deaths were 26, or 1 in 105, while in the following year, 1826, the deliveries were 2,440, 306 less than the previous year, but the deaths were 81, or 1 in 30. In the year 1833 the deliveries were 2,138; deaths 12, or 1 in 178. In the following year, 1834, the first of Dr. E. Kennedy's mastership, the deliveries were 2,024, 114 less than the previous year, while the deaths were 34, or 1 in 60, almost 3 to 1 as compared with the previous years. But why weary the Society by repeating what they can read and study for themselves in the hospital annals.

Great stress has been laid upon the enormous mortality of the Rotunda Hospital during the last fifteen years. This can be explained, however, at least to my satisfaction, and without one particle of that self-condemnation which has so fearfully preyed on Dr. E. K. ever since the time he became Master of the Hospital. First, then, I assert there has been an enormous—an unprecedented—amount of epidemic and zymotic disease during the last fifteen years or more, and in proof of this I appeal to all our hospital reports, as given in the journals; this may be looked upon as vague and uncertain, but a single sentence from one whose authority all present will acknowledge, sets the question at rest. I allude to Dr. Wm. Stokes, the pride and the ornament of our profession. In an address lately given in one of our leading hospitals, he says—"Epidemics have, during the last twenty years, desolated our country." In the next place, it must be patent to

all that the fewer our deliveries in such an institution as the Rotunda Hospital the greater will be our death-rate, which is easily explained thus:—A puerperal epidemic sets in; some patients in the hospital die of the disease; a panic is created; the alarm spreads; the patients are reported as dying by the dozen; all who can possibly stay away shun the hospital as they would the plague-house, none but the abandoned and destitute dare to enter; the epidemic still prevails, deaths multiply, and the hospital is closed perhaps for weeks. It is again reopened, but the patients come in few and far between. The annual report appears with an enormous diminution of patients, but a fearful increase of death-rate; this is easily accounted for, the patients most susceptible of the disease come in, those least susceptible who have comparatively comfortable homes stay away. Thus in 1861 and 1862 we had repeated outbreaks of puerperal fever in the hospital. The hospital was closed for a considerable time, our deliveries were only 800, our deaths 56, or 1 in 14. Let me ask, is the hospital accountable for this deplorable state of things? Was the disease confined to the hospital? I assert there was a considerable spread of epidemic of puerperal over the entire city that winter. One practitioner who has a very extensive practice, chiefly among the poor or middling classes, told me he had 9 deaths, and another that he had seen 4 women die from puerperal that winter. Is it fair or just, I would ask, to base any argument on the statistics of such a year as '62? What would be said of the person who would select a year of cholera such as we have had in Dublin, take the statistics of an hospital that had opened its doors to admit such patients, and then have the deaths from cholera brought up against it in proof of its general mismanagement or unhealthiness? Such has been our fate, such our misfortune. One other proposition of Dr. E. K. I will notice, and only one. "8th. It is not only confined in its occurrence to a given hospital, but it is observed to haunt certain wards of the hospital, and this to such a degree that I have been obliged to close up for many months wards in which it established its special habitat."

My experience is entirely at variance with this proposition, and so, I believe, is that of the present master. I feel assured we never had the puerperal to any great extent in the hospital; that it did not prevail as an epidemic over the city, or, at least, some districts of it; and I solemnly declare that I never saw an instance during the whole term of my mastership in which I could say that I had ever seen one patient take the disease from one already attacked with it. I have seen two or more patients stricken down by the disease in the same ward, nay more, I have seen every patient in the ward attacked with the disease, but they were all seized almost simultaneously. No time for tracking the disease from one to another; no period allowed for incubation of the poison from the time of delivery to the period of seizure. Again and again have I seen women

on either side of a puerperal patient make perfect recoveries, and go out on the 8th or 9th day perfectly well. My explanation of the phenomena is this. The epidemic influence prevails, it seizes on the parturient woman, before her labour comes on, and consequently before she had entered the hospital; there it remains in a latent condition, perhaps for days, as we often see in other cases—for example, common vaccination, or the venereal disease, the woman is delivered, and then the smouldering fire breaks forth.

I by no means deny the infectious or contagious nature of puerperal fever or other zymotic diseases, but I believe it to be greatly exaggerated. Allow me to close my observations by reading an extract from an address delivered by Dr. Wm. Stokes in the Mater Misericordiæ Hospital, some time since, in every word of which I concur.

"It has been objected that patients in fever and affected by other contagious diseases should not be admitted into an hospital of this kind, fearing that the other patients might contract the disease. I am happy to say that I am in a position to give this the most complete refutation. Since the year 1832 the upper story of the Meath Hospital has been devoted to fever cases, and during that great length of time there has not been a single example of the disease having communicated itself to other patients. And you will observe that in these wards we have typhus fever, small-pox, and the various contagious diseases. Nay, more—for the last few years the chronic medical ward has been opened up to the fever ward, and yet there is no reason to suppose that a single patient in the chronic medical ward has been affected by the contagious disease."

Permit me to read an extract from Dr. Spath's report of the Hospital in Vienna for the years 1867–'68:—

"*Puerperal Fever, Vienna*, 1867–'68.—From a careful comparison of statistics of the Vienna Lying-in Hospital it would appear—

"1. That puerperal fever has usually existed at the same time in the city as in the hospital.

"2. Sporadic cases frequently occur in the various wards.

"3. The women often show symptoms of the fever before, or shortly after delivery.

"4. The children show symptoms, or, at least, marks of the fever, and usually do not live.

"In the year 1867, of 3,564 women confined 41 died, part from puerperal fever, and part from other diseases, *i.e.*, 1·15 per cent.

"In the first ten months of this year the state of health was satisfactory, as out of 2,967 cases only 23 died, *i.e.*, 0·77 per cent., and these from various diseases.

"From November 13th puerperal fever began to show itself, and

In November,	out of	329	confined,	11	died,	*i.e.*, 3·34	per cent.
„ December,	„	268	„	7	„ „	2·61	„
From Jan. till 25th, incl.		225	„	5	„ „	2·22	„

"So that of 822 delivered since the fever broke out, 25 died—2·89 per cent."

Also two letters this morning received from two of our most intelligent and talented provincial practitioners:—

"My Dear Denham,—In answer to your inquiries, I beg to say that we have several times had puerperal fever as an epidemic when in the town, also in the country surrounding. I have no statistics to give on the subject, but my memory is very clear, as the cases seem well indented on my brain.

"I have in sorrow to add that my treatment has been very unsuccessful. In fact, I know of no disease of which it may be more truly said thus:—

"'Pallida mors æquo pulsat pede, pauperum tabernas
Regumque turres.'

"I lost a lady last year, when there was no accounting for the access of the disease, save by atmospheric influence. At the same time there were several deaths, which I did not attend, in a circuit of about three to four miles, in a direction opposite from the house to that at which I reside.

"This year I lost one case in the humblest rank of life, although there was not another one in the town; several occurred in the country.

"Of course, when I speak of puerperal fever I speak of the true blood poisoned disease, and not those acute attacks of metritis which occasionally occur, and give way promptly to suitable treatment. I have lost four cases in a month in this village, but certainly the sanitation was at the time far from good; but I never had losses without either hearing or seeing cases amongst the farmers in the country at the same time.—Yours truly,

"J. Martin, Portlaw."

"Dear Dr. Denham,—I cannot recollect a case of puerperal fever, sporadic or epidemic, in this locality, during the last twenty years.

"I have not met one in my practice, public or private. I have consulted some of the midwifery practitioners of the neighbourhood since I got your letter, and they bear testimony to a similar immunity.

"I would remark that we have enjoyed a remarkable freedom from epidemic erysipelas in our hospitals here, as long as I have known this neighbourhood. In our county infirmary there has been no tendency to any outbreak of erysipelas for the last twenty-five years, or thereabouts—beyond that I can't speak. Solitary cases of severe phlegmonous and gangrenous erysipelas have appeared in the surgical wards from time to time, but have not spread beyond the bed of the affected party, or attacked surgical cases in the same ward.

"This I believe to be owing, in great measure, to cleanliness, ventilation, and hygienic precautions.

"I shall here mention that a very remarkable and deadly epidemic of puerperal inflammation traversed *the course of the river Barrow* some twenty years ago. It was very fatal, and seemed to follow the course of the river, seizing on parturient women of all ranks of life who lived within a considerable distance of the stream on either side. It led to considerable mortality in Carlow, and about Bagnalstown, and appeared to follow the course of the river, much as epidemic cholera has been observed to do.

"It did not extend to this locality.

"One very remarkable case occurred at this time. A lady of the upper walk of life died of it, after childbirth, near Bagnalstown. Her aunt by marriage, a matron, who attended her most assiduously to the last, returned home, a distance of some ten miles. She was seized with and died of a like attack! She was not, I rather think, pregnant at the time. She was the mother of several children, but had not been lately confined.

"I would obtain further information and consult the past experience of the older obstetricians of this neighbourhood, only that you request me to reply by return of post, which leaves me no time to compare notes with brother practitioners. I think all about here would bear the same testimony.—Yours sincerely,

"Zach. Johnston, Kilkenny."

The two letters I have read are of very great importance, and well calculated to throw light upon the subject now under discussion. It is quite clear that if Dr. Johnson had had a midwifery hospital in Kilkenny, the death-rate from puerperal would have been even less than 1 in 1,200, in fact nil; whereas, had our friend Dr. Martin, in Portlaw, been blessed—or rather cursed—with even a small one, his death-rate, in all probability, would have exceeded our most fatal years. Only think of four deaths in one month in a small town, but not in hospital, more than Dr. E. K. had in 3,500 deliveries. This is a difficult question to solve, but it can be accounted for to a great extent by the fact that Portlaw contains a large manufacturing population; Kilkenny, as far as I am aware, is free from so fertile a source of zymotic disease.

I have taken up much of the time of the Society, and yet I feel as if I had not said one-half what I wished. My experience in the treatment of the disease is altogether at variance with that of Dr. E. K.; for instance, he says, for true puerperal fever of the present day, local depletion must be our sheet-anchor; relays of leeches, in fact, every six or eight hours. For the cases I have seen, such heroic treatment would unquestionably have added considerably to our death-rate. Again, he says, where peritonitis or metria were to be anticipated, my habit was to commence at once after the labour was completed with small and frequently-repeated doses of mercury, and even applying, in more suspicious cases, mercurial ointment to the armpits and groins. To mercurialize a patient

merely because she *might* take ill appears to me to be both unphilosophical and unjustifiable.

And now, sir, one word in conclusion. Could some large-hearted philanthropist be induced to attempt the improvement of the social and moral aspect of the lower classes in our city, and could they be induced, among other things, to build numerous rows of small houses in some healthy locality, I believe they would do more to banish puerperal fever from our city and our hospitals than all the Utopian, impracticable plans of cottage hospitals ever propounded. We may pull down our hospitals, we may destroy the prestige of our city as a school of medicine, we may build cottages, ornèes, having all the pleasing effect of the Swiss chalet, but until we improve the condition of our starving population, and lift them up out of the moral and literal cess-pools in which thousands of them wallow, thereby rendering them less susceptible of zymotic poison, we shall have puerperal fever as long as humanity is debased by poverty, drunkenness, and crime.

An adjourned meeting of this Society was held on Saturday evening, the 22nd May, in the Royal College of Physicians, Kildare-street, and the debate on Dr. Evory Kennedy's paper on "Puerperal Fevers" was resumed.

DR. RINGLAND occupied the chair.

DR. KIDD, Secretary, read the minutes of the last meeting, which were confirmed.

The CHAIRMAN announced, that in addition to the members of the Society who might wish to speak in the course of this discussion, he would be happy to hear, also, the observations of any other medical gentleman present. Dr. Sawyer had moved the adjournment of the debate, and he would now call upon him to proceed with his remarks.

DR. SAWYER then rose and said:—Mr. President and Gentlemen, before I begin the only point that I shall wish to refer to is this:—I have been requested to call the attention of the Society to an error in the calculations of my friend Dr. Kennedy. At page 290 of his paper he has a calculation prepared from the deaths and births of children in the registration districts, and in the Coombe and Rotunda Lying-in Hospitals for five years, and he has stated that the children born in the Coombe Hospital were 2,266, whereas the actual number was 2,366, making a difference of 100 children—so far invalidating his whole statement. Well, in this discussion, carried on, as we may say, "war to the knife," 100 is something, and, therefore, though it looks very small to the eye where we are fighting the battle so closely, it was very right of Dr. Kidd to remind me of it. I come before you with very great regret, mingled with satisfaction, that I am privileged to take part in this important discussion,

which, I assert, is the most practical that has ever occupied the Society since its formation in 1838. (Hear, hear.) I need scarcely, gentlemen, allude to the fact, that Dr. Kennedy has been the respected teacher of many of us, and that he has been the founder of this society. (Hear, hear.) Therefore, you can readily enter into my feelings when I declare that if I have to say things that may be disagreeable to Dr. Kennedy, I shall regret it; but the topic is far too important, the subject-matter is of too great consequence, to allow any private feelings to interfere between speakers in this discussion. [Dr. Kennedy—"Hear, hear."] And yet, gentlemen, I am at the same time rejoiced that this opportunity has been afforded me of speaking for the hospital of which I have been for twenty-two years one of the masters, an institution that, in connexion with the Rotunda, has, up to the present day, enjoyed the unlimited confidence of the inhabitants of Dublin and of our professional brethren. (Hear, hear.) Therefore, it grieved me to find that there had been rumours spreading—sidewinds—that these hospitals that we managed, or thought we had managed, to the best of our ability—conscientiously, and, permit me to say, effectively—to find that these institutions were to be spoken of as "pest-houses," as "charnel-houses," and places to which the unfortunate females that could not purchase skill were compelled to resort, absolutely with the risk of death. Now, gentlemen, a great deal has been said, and well and ably said, on the subject. I never recollect, and I think you will all agree with me, that there never has been a discussion that has elicited more consummate skill, more admirable argument, or more disinterested support, as far as any man can see, with reference to these two institutions—the Coombe and Rotunda Hospitals. We are thankful, then—at least I am, on behalf of the Coombe; as for the Rotunda, it has been grandly represented. A phalanx of men has come forward who have borne honourable and faithful testimony to the efficiency of that institution, to the value of the aid it has afforded to the poor of the metropolis. But I regret the source from which the blow has been struck. I more than regret that, though my respected friend and teacher asserts that for thirty years this has weighed upon his mind. I regret that he did not make it thirty-one years, and in that one year have taken counsel with us. He has no cause of complaint against us. Our hospitals have been open to him, and when he asked us for any statistics, or for any copies of our registries, they were all freely and willingly open to him. And yet, after all, I say that he has come forward alone—alone! I maintain it, gentlemen, he has not the co-operation of his brethren—he has not! He comes exactly on his own responsibility, and open to this question—Is it for reformation? is it for alteration? is it for suggestion? is it for sanitary improvement? No; but destruction!—*delenda est Carthago.* It is nothing else. When I think that for twenty-two

years I have been in such a position that three out of four have died in the Coombe Hospital, and seven out of eight have died in the Rotunda Hospital, what an involuntary homicide I must appear before you. (Hear, hear.) Now, gentlemen, you will infer that these establishments, although we have been the involuntary agents of keeping them up—admitting that they are as described—must at least have called forth a remedy equal to remove the lamentable errors that have been so commented upon. I think that if, admitting that we, by our intense ignorance of the laws of metria, have gone on still leaving the portals of the hospitals open to the poor ignorant wretches seeking, misguided, to be admitted in the hour of their greatest need and misery, and find that after all they were literally doomed, it is something terrible to think about. I will just simply say this—that you expect that Dr. Kennedy, having decided that these terrible evils, these frightful grievances, have existed for so many years, and in a spirit of Christian liberality he himself has lamented it—lamented it for thirty years, and kept all these within his own heart until absolutely he could not keep them any longer—any longer! (Applause, and laughter.) But he is going, as he says, to his account. (Laughter.) God forbid! His remarks are, and he almost made me cry when I heard them—"As a governor and ex-master of the Lying-in Hospital my impressions have been confirmed as to the necessity of the change I now advocate. I feel that with my knowledge and convictions silence on this subject would be cruel heartlessness, not to say criminal. (Hear, hear.) This feeling of dissatisfaction increased with age and experience. So do our apathy and listlessness. Life runs on apace, and misery and redress of wrong lag. These convictions must plead my excuse for so tardily calling public attention from the things of our common profession to these great evils." That is beautiful writing. (Laughter.) Having read so much, and feeling almost as a condemned criminal, and not having committed a murder voluntarily, I will just simply come to the point, and give what Dr. Kennedy suggests as a redress. Honestly, I thought there was to be a revolution in my ideas of everything I learned. I went on reading until I came to this conclusion—that lying-in hospitals having, according to Dr. Kennedy, been proved to be hotbeds of disease and instruments of destruction (that we thought were something so different), he should suggest some better plan. What was it? Dr. Kennedy knows what I mean, and it is a beautiful idea. I shall return to the paper and quote it. Mark you, this suggestion has been made by Dr. Kennedy before he has done us the honour of submitting it to the Obstetrical Society. I think it was in the shape of a letter, but I forgot to bring it. However, you all know the celebrated letter. It was addressed to the Governors of the Rotunda Lying-in Hospital, in which, having pointed out the facts that he puts down here, he asserts that these evils are to be met by breaking up the

Rotunda *in toto*; by dividing it into two separate buildings—one institution for the education of midwives, and the other an hospital for the diseases of women and children; and then—I don't know whether he excluded the master—but we were to have thirty huts, or chalets, or pavilions in Rutland-square, and the master was—he looks very reconciled I see—*bonâ fide* to give up half his fees and be content with £500 a year. (Laughter.) Then we were to have this complete isolation of buildings—every hut having but its two inmates—which I presume would make sixty—at all times ready for carrying on this idea, with Dr. Johnston and £500 a year to superintend it. This, gentlemen, I don't know why and I cannot tell, was not agreed to by the governors of the Rotunda Hospital. It was not, I honestly tell you. Then my respected friend has given us these: "Zymotic diseases, as more especially illustrative of puerperal fevers; by Evory Kennedy, M.D., ex-Master of the Lying-in Hospital, and ex-President of the Royal College of Physicians." This is 1869; but I think the original paper was in 1867. That was the time I had the honour of being president of this Society. Well, now, this is a most excellent paper; the fact is that nothing that Dr. Kennedy ever wrote could be anything else. I may well say, *Quicquid tetigit ornavit.* No man can take up this paper without feeling that it is *bonâ fide* from a man of thoroughly sound knowledge of his profession. I firmly believe that there is no one in this room that will not endorse my idea. (Hear, hear.) I hope Dr. Kennedy will excuse me in saying that my belief is that if that letter to the governors had been acted on as it was intended, we never would have had this debate,—this paper would have been still-born. (Laughter.) We never would have had this treatise on zymotic diseases.

On the last evening I had my old pupil and my respected friend, the Lord Mayor, here. When we were going away, he said to me in his own pleasant way, "You are all down on poor Kennedy." (Laughter). I said—"Well, that is what a man has to pay for his position in society. If such ideas emanated from any other we would have just let them pass over." Here is a man we all delighted to honour, and I say it, however it may tell against me—and it will to some extent—there never was a master of the Lying-in Hospital, or practitioner in midwifery, that has more claims to the respect and honour of his brethren than Dr. Evory Kennedy. (Applause.) Therefore I said to the Lord Mayor:—"My Lord—In proportion as a man holds a high position, if we consider his doctrine erroneous, we pitch into him, and the harder we hit him the more we show our respect and affection for him." (Hear, hear, and laughter.) I do not think my most excellent friend would have been so desperately hard on these hospitals had he taken counsel with us. I think that would have been a great deal better, and, God knows, I would not have hurt his feelings—if I am hurting them—if he had brought this

paper and submitted it to discussion, in connexion with what he was proud to say was the model Society of Dublin. There is never any unkindness, or antagonism at our meetings. But we all come here for the ascertainment of the one point—truth. In every discussion our object is to elicit truth, and nothing else, and therefore I do regret that my friend, Dr. Kennedy, did not come. The remark, I think, was made by Dr. Madden, that he regretted that Dr. Kennedy had not delayed somewhat longer in coming before us. Very well. Now, what was his answer? [I don't want to say a single thing that I have not chapter and verse for.] Dr. Kennedy said simply that he had been thinking of it almost too long. Almost too long! I will just get this point—but I believe I forgot my spectacles—

Dr. Kennedy.—Would mine be of any use to you? (Laughter.)

Dr. Sawyer.—Well really, doctor, thank you, I am afraid we don't see through the same glasses. (Laughter.)

Dr. Kennedy.—More is the pity.

Dr. Sawyer.—More *is* the pity. Dr. Kennedy's remarks were simply to this effect: that he pulls my friend up—and serve him right—for remarking to him that he should have delayed a little longer. The point is this, that all he had done was to endeavour to throw light upon the matter, as far as he could, after an experience of forty years. I wish he could have extended it to forty-one years. I take the liberty of asking my friend, Dr. Kennedy, to permit me to use a little of that jocularity that made us all laugh so. He says here: All he had done was to endeavour to throw light upon it after an experience of forty years, and one gentleman said he wished he had considered it a little longer. He had been considering it, and spending so many anxious days and nights over it, and when he was asked to consider it a little longer, he thought that was unreasonable? But to go back to the hospitals. Now, gentlemen, this forty years' period has put an idea into my head, and it looks to me in this way, that as Moses of old was wandering for forty years through the wilderness, and then was permitted to see the promised land—(laughter)—so my respected friend has looked out upon the promised ——

Dr. Kennedy.—You mean Moses?

Dr. Sawyer.—Not Moses more than yourself. He has looked out upon the promised land, and he has seen the whole area of Rutland-square, spread out with huts, or pavilions, or Swiss chalets. That is something, and I have made it come forward to illustrate Dr. Kennedy's forty years of travelling in the wilderness. You all know what the Coombe is. You all know what the Rotunda Hospital is. Friends are equally pretty well divided, and you know, gentlemen, that all that science, and experience, and money can *bonâ fide* bring to bear on any point to comfort the suffering poor of Dublin has been accomplished. I appeal to you all, or to any one of you that knows these two hospitals,

can you find any point in the whole range of modern appliances in which they are deficient. (Hear, hear.) Now let us see what we are to have in lieu of them. He proposes that one part of the Rotunda be allocated to the diseases of women and children, and that the other portion be for the education of midwives; and that the entire circle of Rutland-square be dotted over with huts—called huts, if you are humble; pavilions, if you are fashionable, or the chalets of the Swiss. Thirty of them are to be there in a circle, and over them my respected friend, Dr. Johnston, is to be the presiding genius. Now, mark me, gentlemen, to make the principle of isolation perfect, every hut must be on its own ground separate. Then just fancy the doctor of the hospital paying his morning round of visits, in the wet or rain perhaps—I presume he will be allowed an umbrella—with his assistants; with, I presume, his midwives, and with his forty pupils. Why the Grand Signor never was better attended. Of course this may be very ridiculous; but I say it to show that it is impracticable. If there is to be isolation, there must be no yard passage, no corridor—it must be complete and perfect. I appeal to you, or any of you that have attended hospital, might we not have in the middle of the night, "No. 15, hemorrhage"—I presume there will be a telegraph; I hope or suggest there will be something of the kind—"bleeding." "No. 18, forceps." "No. 25, craniotomy." We may all think differently of course; but can any of you idealize this; that having regard to the attending to the interests of the poor of the city of Dublin, can there be any such arrangement? (Hear, hear.) Now, gentlemen, I don't for a moment object to the theory, nor do my respected colleagues, Drs. Ringland and Kidd. We all of us admit that isolation of women in parturition is one of the most desirable things to be effected. I fearlessly and fairly put that forward; but the grand point is to be practical, to be simple, and to be adaptive, if we mean to carry it out. I just simply say this that as far as I can understand at present, that this separation of the Rotunda into isolated huts of two patients in each, with one nurse and one ward-maid, and, I presume, another servant for every four or five huts, would be one of the most difficult complications, and one that I hardly think the Rotunda is prepared for. I don't think we are prepared for it either. I simply put it to you practically, after twenty-two years' connexion with the Coombe Hospital, I could not see it; I could not understand it, nor could I endorse it, although my opinion never was asked, so that my opinion goes for nothing. But what would the inhabitants of the square think of this plan? Why already they are objecting to Dr. Johnston's providing them with music. Already they are threatening an action on account of Mr. Hengler's Cirque. But just imagine all the operations of midwifery going on in these thirty huts. Let us look at it practically, and I will be delighted if my respected friend will illustrate that point. I am afraid he won't as

I look at him. This plan of huts has been very contemptuously spoken of—I don't insinuate for a moment what they were called, but I do say that the plan, if carried out, would look excessively like a rabbit warren, without the connecting corridor which the warren has for the convenience of the community. Dr. Kennedy will take the description of a number of isolated huts, with no connecting passage, for if there is any kind of corridor his plan is done away with. You cannot have complete isolation unless you have open air between each hut. We shall leave this point and turn to another. We have next to consider a terrible point. I think it is impossible to conceive, if I have read aright—I can scarcely conceive I have read aright—and that it is his deliberate conviction which he has expressed with reference to the mortality of these hospitals. The point on which we are on trial is simply this: that in the Coombe Hospital, by the present calculation, three out of four die that should not; and that in the Rotunda Hospital—and that is the only consolation I have—eight out of nine die that should not. Truly, I may say with the song of the maidens of Israel:—"Saul hath slain his thousands, and David his tens of thousands." (Laughter.)

A grave accusation has been brought forward by my respected friend, a member of our profession, and, need I say, gentlemen, that the masters of these hospitals have to come forward! Need I say that we have cause of complaint against Dr. Kennedy? and I say that we have cause of complaint against him. What is it? Why in this very paper, how does he begin it? "Let us take sweet counsel together." (Laughter.) No; it is simply this: He invites us the members of the Obstetric Society,—he invites us to join in the discussion on this very terrible, this awful, this mysterious pestilence—(hear, hear),—that goes about absolutely and literally as the pestilence that walketh in darkness and the sickness that destroyeth in the noon day. I say it, that if Dr. Kennedy had not prejudged this—prejudged it! and if he had come with us into counsel together, and had given us the statistics, and any other information he had, I for one would have thrown up the hospital if they could not have been refuted. But I have a decision here, and what is it? Is it my decision? No, for I am interested. Is it the secretary's or the president's decision? No; they are interested. The decision is before me. You, gentlemen, are my jury, and honestly would I throw down all my interests in the Coombe—I would lay all my pecuniary interests before you, if it is against me. I would say, "Give us your verdict, either for or against, and we will abide by it;" and I will ask my respected friend, if he does not agree with me, that we were perfectly open to his visits, and nothing would have gratified me more than to have gone round with him to investigate the working of the hospital. I know that my friend Dr. Johnston agrees with me, but he can speak for himself. I would say that if it can be shown that a better institution for the poor

of the Liberties of Dublin can be established, or that by any other plan there can be more lives saved, then, in God's name, let the Coombe go. I speak of this as I think I ought to speak. I think this matter has been put before the public wrong, and invidiously so. If the author of these statements had *bonâ fide* exerted himself in this matter, and brought it before the Society, they would have considered it; but not having done so, he has compelled me to come before the public to show that it cannot be done. I am not begging the question here. I ask you to judge of the Coombe and to judge of the Rotunda on their merits. Don't judge of them by one man, for what man is infallible? Let us be judged by our peers. Let us be brought forward for trial, and I say that I shall stand or fall by that verdict. In this most remarkable document he invites us to this discussion, and he goes on to say—and remark what a clever man he is:—"We may state it to be between one-fourth and one-fifth of the gross mortality. After that it may be asserted, for argument's sake, there is a danger that zymotic diseases result in nine out of ten cases from preventible causes." Would any of us allow him to come to any such conclusion "for argument's sake?" It is impossible to come to such an awful conclusion, because no criminal is called upon to admit his guilt, even supposing he was guilty. He calls us to this bar of public opinion, to be judged by public opinion and his own, and he wishes us for argument's sake to admit that zymotic diseases result in nine out of ten cases from preventible causes. That would be very dangerous to admit, I tell you. Then he goes on in an admirable manner, and I must thank Dr. Kennedy for it, for I have gained a great deal of information from this paper; but I don't like the object of it. Now, then, we come simply to this point of isomerism, that one grand-parent of zymotic diseases, being developed into an awful family of children——

A Member.—Twins. (Laughter.)

Dr. Sawyer.—Far more than that—triplets.

The Chairman hoped they would have no further interruption from this by-talk.

Dr. Sawyer.—He says:—"This zymotic disease prevails in the hospital, in which it is seen in its greatest virulence, excepting those most constantly ventilated." Then he honestly says: "It has also been met with in the hovels of the poor, and even amongst the dwellings of the wealthy." That is all right. He then goes on to the laws of zymotic diseases, as typhus fever, cholera, scarlatina, &c.; but this has been so admirably commented upon by my friend, Dr. M'Clintock, that I will not trouble you further. And now, gentlemen, we come to practice. It is most curious, and I cannot get out of my mind, the idea that the better part of the treatise was composed thirty years ago. There is certainly something antiquated as to the treatment. I served my time in the Meath Hospital, and the Cork-street Hospital. That was the time of

bleeding; the public bled, and the patients bled. Dr. Mills and Dr. Stoker, and all, were for bleeding. Clearly I admit that puerperal was of a sthenic type, and therefore Dr. Kennedy's treatment in reference to this special time was judicious. It was relays of leeches and mercury; mercury, in suspicious cases, was commenced almost immediately after delivery. That treatment was taught by Dr. Johnston as well as by Dr. Kennedy, and they were accustomed to teach that nothing was better than small doses of mercury that we could hold in our hands, if no symptoms supervened to push it on. That was very excellent treatment in that period—1838. But what is the treatment in this day? I have here a pamphlet by Dr. Graily Hewitt of London, and let us see what he says as to the treatment of 1869. Leaving aside his theories about the origin of metria, which are as numerous as the days of the month, I will say that it is the mode which I, Dr. Kidd, and Dr. Ringland have been in the habit of employing, and which is the only reliable treatment we have faith in. Dr. Hewitt says, after describing the great pains taken in the article of diet:—"The patient was at once ordered to take a draught composed of thirty minims each of tinct. lav. co., mixt. camph. co., sp. ammon. arom. eth. chlor., and a little camphor mixture. This red mixture was given every four hours or oftener. Brandy diluted with water, or mixed with yolk of egg, was ordered in quantities varying according to the case. In a mild case six or eight ozs. per diem would be given, but in some cases I have ordered as much as two ozs. of brandy every two hours during a consecutive period of twelve hours." We all know that. I have drawn your attention to this for another object. During the last fifteen years we have had such a remarkable tendency to metria; but I say, take away all ideas from metria, and go abroad, look outside the hospitals, and answer me, is not debility the order of the day? I ask the practitioners here is not debility the order of the day? Can we, in treating inflammatory affections—pneumonia, bronchitis, &c.—can we do more than blister? But as for leeching—can we employ it? With reference to Dr. Kennedy's treatment. When I was attending the hospital there was a remarkable case that struck me. It was at a time when leeches reached the price of £3 per 100, and there was one case there when the hospital was without leeches. The woman was not leeched, and she recovered (laughter). Really, I am afraid I am wearying you (no), and I am not inclined to occupy your time. But still this is one of those cases in which, if a man has anything to contribute, in God's name, let him contribute it. My own feeling was that I should not have come before you to-night. It occurred to me that "lumping" the deaths in an hospital told terribly against that hospital with our adversaries, or whatever you may call them. Now, what is the grand contrast? It is between the deaths within and the deaths without the hospital. It may be said, "There you

are in the hospital, and you are murdering the patients; but with those outside, thank God, they are escaping." Is that the fact, or is it not? That is the question before us. I would not come before you to-night if I was not prepared to say how the hospitals were doing their work. I would say to these people, "Give us something better, or let the hospitals alone until you do." Surely, that is not too much to ask from you. I have got the Registrar of the Coombe Hospital to give me statistics—not statistics, indeed, but copies from the books of the Coombe Hospital from January, 1861, to December, 1868. I had this only within the last two days, and Mr. Weir just writes to me thus:—

"Coombe Hospital.

"Dear Sir,—I have taken from the register the deaths which occurred from January, 1861, to December, 1868—eight years—which you will find on the accompanying sheet. In each of the cases I have given you the name, residence, stage of illness, age, the operation, and whether in the second or third stage, and cause of death. I find there were admitted into the hospital during that time 4,091 cases, 374 of which were not in labour, leaving 3,717 *bonâ fide* labour cases. Of this latter number 54 died from all causes, or, one in 69 nearly. Puerperal fever in varied forms caused the death of 30 patients; other causes, such as hemorrhage, convulsions, &c., occasioned 24 deaths. In the great majority of fever cases the second stage was unusually long, and also the age of the patient nearly up to or beyond 39 years."

I think that is a *bonâ fide*, interesting, and instructive document (hear, hear). I put it in for the inspection of you all. Here I find that of those fifty-four cases of deaths by metria—but there are not fifty-four cases of death from it—there is a most remarkable amount of complications of causes turning up to prevent the natural labour, so that, I think, will greatly modify your opinion as to the death-rate in our hospital. Of these fifty-four cases ten cases were forceps, five retained placenta, four version, three were cases of bronchitis, and there were admitted in a dying state four women (hear, hear):—

Deaths in Coombe Lying-in Hospital from 1861 *to* 1868.

No.	Name	Residence	Age	Length of stages			If married	Presentation	Operation		Date	Cause of Death	Observations
				1	2	3			2nd Stage	3rd stage			
				H.	H.	M.							
1	Margaret Toole	Clonskeagh	30	48	10	5	M.	H.	Craniotomy	—	Jan. 1861	Ruptured uterus	
2	Fanny Byrne	Golden-lane	30	13	6	10	M.	H.	Forceps	—	March 1861	Diffuse bronchitis	Forceps, by Dr. Kidd
3	Mary Kinsella	Corn Market	20	2	2	10	M.	H.	Forceps	—	March 1861	Convulsions	Forceps, by Dr. Kidd – first confinement
4	Mary Whelan	Harold's-cross	23	8	4	20	M.	H.	—	—	March 1861	Mania	First confinement
5	Anne Maher	Edge's-court	25	3	1	30	M.	H.	—	—	May 1861	Puerperal Fever	Typhoid symptoms set in soon after labour
6	Eliza Boyne	Great Ship-street	35	20	1	20	M.	H.	—	—	May 1861	Puerperal Fever	Do. do.
7	Mary Clithero	Harold's-cross	36	2	½	20	M.	H.	—	—	October 1861	Syncope	This woman walked hurriedly from Harold's-cross, had a rapid labour, shortly after which she rapidly sank
8	Anne Doyle	Blessington	23	5	1	25	Not	H.	—	—	Jan. 1862	Puerperal Fever	
9	Eliza Ledger	Fleet-street	22	9	2	20	M.	H.	—	—	Feb. 1862	Puerperal Fever	
10	Mary Langly	Mercer-street	30	4	3	10	M.	H.	—	—	Feb. 1862	Puerperal mania	
11	Julia Creman	Meath-street	28	8	3	20	M.	H.	—	—	March 1862	Puerperal Fever	
12	Mary Murphy	Dominick-street	32	7	2	40	M.	H.	—	—	May 1862	Puerperal Fever	
13	Mary Byrne	Greek-street	30	—	—	—	Not	H.	—	—	April 1862	Puerperal Fever	
14	Jane Rowlands	Temple-bar	42	6	4	20	M.	H.	—	—	June 1862	Hemorrhage	
15	Catherine Moore	New Market	30	30	5	30	M.	H.	—	—	Feb. 1863	Debility	Debility from starvation
16	Jane Kells	Back-lane	26	16	4	30	M.	H.	—	—	Feb. 1863	Debility	
17	Eliza Murphy	Bride-street	40	—	—	—	M.	H.	Craniotomy	Removed	May 1863	Peritonitis	Craniotomy, by Drs. Johns, Ringland, and Kidd—a long time in second stage when admitted

Deaths in Coombe Lying-in Hospital from 1861 *to* 1868*—continued.*

No.	Name	Residence	Age	Length of stages			If married	Presentation	Operation		Date	Cause of Death	Observations
				1	2	3			2nd stage	3rd stage			
				H.	H.	M							
18	Mary M'Govran	Harold's-cross	32	48	10	30	M.	H.	—	—	July 1863	Fever	Admitted in a dying state, and sank 3 hours after admission
19	Elizabeth Murphy	Pembroke-quay	43	32	4	10	M.	H.	Forceps	—	August 1863	Peritonitis	Forceps, by Dr. Mason
20	Margaret Ellis	New-street	21	9	11	15	M.	H.	Forceps	—	October 1863	Peritonitis	Forceps, by Dr. Kidd
21	Margaret Smyth	Cuffe-street	27	12	8	10	M.	H.	Forceps	—	Nov. 1863	Convulsions	A first case—delivered by Dr. Kidd—had convulsions when admitted
22	Catherine Cunningham	Coal-alley	37	2	—	—	Not	Hand	Version	—	March 1864	Convulsions	Version, by Dr. Kidd
23	Catherine Bryan	Nicholas-street	27	18	5	20	M.	H.	Craniotomy	—	May 1864	Peritonitis	Craniotomy, by Dr. Ringland
24	Mary Mulligan	New-street	38	7	¾	10	M.	H.	—	—	Sept. 1864	Puerperal Fever	Had severe accidental hæmorrhage before admission
25	Mary Colgan	Great Longford-street	23	23	5	10	M.	H.	—	—	Jan. 1865	Peritonitis	
26	Catherine Norman	George's-avenue	25	—	—	—	Not	H.	—	—	Feb. 1865	Acute bronchitis	First case—had convulsions after labour, from which she had recovered
27	Mary Knowls	Winetavern-street	23	—	—	—	M.	H.	—	—	Feb. 1865	Fever	Dying when admitted
28	Anne Brunton	Meath-street	33	24	5	10	M.	H.	Forceps	—	July 1865	Peritonitis	Forceps, by Dr. Ringland
29	Mary Dunne	Francis-street	28	10	2	10	M	H.	—	—	Feb. 1866	Convulsions	
30	Catherine Clarke	South King-street	32	—	—	—	M.	H.	—	—	May 1866	Phlebitis	
31	Anne Flood	Blackrock	30	8	5	25	M.	H.	Version	Removed	May 1866	Phlebitis	Brought in an open cart from Blackrock in a state of coma from convulsions—placenta morbidly adherent—version, by Dr. Kidd
32	Catherine O'Keefe	George's-place	40	3	2	5	M	H.	—	—	Sept. 1866	Peritonitis	An epileptic
33	Bridget Lyons	Aungier-street	38	24	4	10	Not	H.	—	—	Sept. 1866	Hysteritis	Mania supervened some days before death
34	Jane Rafter	Meath-street	27	23	9	5	M.	H.	Forceps	—	Sept. 1866	Typhus Fever	In fever when admitted

Deaths in Coombe Lying-in Hospital from 1861 *to* 1868—*continued.*

No.	Name	Residence	Age	Length of stages			If married	Presentation	Operation		Date	Cause of Death	Observations
				1	2	3			2nd stage	3rd stage			
				H.	H.	M.							
35	Ellen Keegan	Earl-street	22	10	4	20	M.	H.	—	—	Nov. 1866	Peritonitis	
36	Mary Donovan	Brown-street	27	—	—	—	M.	—	—	—	Nov. 1866	Cholera	This woman was not in labour
37	Mary Burke	Myler's-alley	29	8	2	10	M.	H.	—	—	Dec. 1866	Ruptured uterus	
38	Anne Murphy	Skinner's-alley	24	16	3	25	Not	H.	—	—	Dec. 1866	Peritonitis	
39	Mary Fox	Bride's-alley	26	12	6	15	M.	H.	—	—	Jan. 1867	Typhus fever	
40	Margaret Harrington	Coombe	26	14	6	30	M.	H.	—	—	Feb. 1867	Bronchitis	
41	Eliza Cassidy	Ship-street	34	—	—	—	M.	Placenta	—	—	Feb. 1867	Placenta previa	Died undelivered
42	Eliza Behan	John's-lane	26	12	4	—	M.	H.	—	Removed	April 1867	Subacute peritonitis	Placenta morbidly adherent
43	Hannah Boland	7, Longford-lane	26	10	6	5	M.	Arm	Version	—	May 1867	Peritonitis	Version, by Dr. Sawyer
44	Mary A. Clynch	Coombe	18	30	8	10	M.	H.	Cephalotripsy	---	June 1867	Peritonitis	Cephalotripsy, by Drs. Ringland and Kidd
45	Alice Larkin	Cook-street	28	4	1	15	M.	B.	—	—	July 1867	Phlebitis	Partial placenta previa
46	Jane Whelan	Rathmines	39	30	2	10	M.	H.	Cephalotripsy	—	October 1867	Peritonitis	Cephalotripsy, by Drs. Ringland and Kidd—a case of exostosis
47	Eliza Doolan	Pool-street	25	36	1	10	M.	Arm	Evisceration	—	October 1867	Pleuritis	Evisceration, by Drs. Sawyer, Ringland, and Churchill
48	Julia Gray	Lower Kevin-street	39	10	4	15	M.	Shoulder	Version	—	Nov. 1867	Phlebitis	Version, by Dr. Kidd
49	Theresa M'Cabe	Cathedral-lane	18	8	2	5	M.	Shoulder	Version	—	Dec. 1867	Convulsions	Version, by Dr. Sawyer
50	Ellen Delargy	Whitefriar-street	28	36	2	15	M.	H.	—	Removed	Jan. 1868	Syncope	Placenta morbidly adherent
51	Susan M'Donnell	Marrowbone-lane	34	6	2	35	M.	H.	—	—	Feb. 1868	Pyemia	
52	Mary Mulvanny	Fisher's-lane	33	4	7	15	M.	H.	—	—	May 1868	Peritonitis	Twins
53	Catherine Finley	Blackhall-row	34	10	3	20	M.	B.	—	—	May 1868	Bronchitis	From which she was suffering when admitted
54	Margaret Connor	Coombe	28	30	4	20	Not	H.	Forceps	—	Dec. 1868	Phlebitis	An idiot

Now, on reading over this formidable list of fifty-four deaths and their causes, you will hardly think that the hospital was culpable for so many. Let me read you some, commencing at the foot of the table, No. 54, "An idiot; first child; unmarried." We know that in the first child women are more liable to metria. No. 53, "was suffering from very bad bronchitis when admitted." No. 52, "Twins"—died of peritonitis. The next is No. 51, pyemia. No. 50, "The placenta morbidly adherent." Dr. Kennedy will admit that he never had a case of morbid adhesion of the placenta in which he could guarantee a woman's safety. In that case, at least, that woman's death could not be charged to the hospital. The next is to my account, "Version by Dr. Sawyer." Next, "Version by Dr. Kidd." Next, "Eviscerated by Drs. Sawyer, Ringland, and Churchill;" but I don't know whether it was the woman or the child; it does not say here. One comfort is, that I find Dr. Churchill was present. The next (No. 46) is "a case of exostosis," and one of the most dreadful. Next, "Partial placenta previa; had considerable hemorrhage before admitted." Next, "Cephalotripsy, by Drs. Ringland and Kidd." There were cases that have never engrossed observation. This is from bleeding, and here is a case in which the patient lost a very large amount of blood before admission into the hospital. Then we have No. 43: "Version by Dr. Sawyer." Next, "Placenta morbidly adherent." Next, "Placenta previa; died undelivered." All these I am reading for your instruction, and to enable you to form a judgment, and to understand how far the hospital or the disease can be charged with crime. No. 41, in fact, died as she was being brought into the hospital. Then we have No. 39, which "Dr. Ringland said was one of the worst cases of typhus he had ever seen." Next, "softened state of the uterine wall." Then a woman who "was not in labour, died shortly after admission." Then, "forceps by Dr. Ringland (she had typhus rash on day after delivery)." Next, "mania supervened some days before death." The next, "was subject to epilepsy, and had a fit three days after her natural labour, after which she showed symptoms of peritonitis." No. 28, "forceps, by Dr. Ringland." Next we have—"This patient was dying when admitted." Next, "A first case—had convulsions after labour, from which she had recovered." No. 24, "had some accidental hemorrhage for fourteen days before admission. Very low." Next, craniotomy, by Dr. Ringland." Next, "version, by Dr. Kidd." Next, "delivered by Dr. Kidd—a first case; had convulsions on admission, and never rallied." Then, "forceps, by Dr. Kidd." Next, "forceps by Dr. Mason." The next was "admitted in a dying state." Next, "craniotomy, by Drs. Johns, Ringland, and Kidd—a long time in the second stage before admitted." Next, "in a dying state when admitted." Then again, "In a dying state when

admitted." These are the points on which I wish Dr. Kennedy to give us fair and honourable treatment. Let us be accountable for patients admitted into our hospital that are placed in the terrible position to get metria by contagion, but let no other point be brought against us. Here now, I come upon one of those outbreaks, in 1862, when we had five fatal cases of this disease in succession. If Dr. Kennedy wishes, I will be most happy to let him have this return. It is there as it was taken from our books, and any man is able to corroborate it. I have now almost come to the end of my very wearisome discourse. The question now is this:—What has been done? I think we have done the state some service. (Hear, hear.) This much I wish to say that I had prepared, as I thought, a very admirable account of statistics; but you will all admit that the master of statistics is sitting there (Dr. M'Clintock). I say it was absolutely a privilege to listen to him. (Hear, hear.) They were so clear, and so admirably brought out, and at the same time with such thorough courtesy—with such temperate care not to give offence—that I wish I could imitate him. There is no use in my following him with my statistics, and I think my respected friend, Dr. Kennedy, would not like to have a battle with him again on that point. They were absolutely so intensely conclusive, that they left nothing to be said in reply. Now, for my own part, my idea of statistics is simply this—that unless a man has entered most remarkably careful into them he puts himself in the position of allowing a fall to be taken out of him. You all recollect how admirably my friend, Dr. Kennedy, came out on that point. My idea of statistics is that they are like a Congreve rocket, because when you fire it, who will guarantee that it will not come back on your own head, or that you, in using them, would not be like the engineer who was "hoist with his own petard." Methinks Dr. Kennedy might with Sir Andrew Aguecheek exclaim, "Confound him; an I thought he had been valiant, and so cunning in fence, I would have seen him damned ere I'd have challenged him." (Laughter). There is a new era inaugurated by Dr. Kennedy as to statistics, Registrar-Generals, Phelans, Sausage-ean, Babbage-ean, and Kaleidoscope-ean statistics. But Dr. M'Clintock's were enough, and affected the stomach of Dr. Kennedy so much, that he believed that he had got his sausages from the wrong girl. (Laughter). There is no use in my going over trodden ground, and I am afraid I must have wearied you with this long speech; but still I say, and will maintain, that the object of this discussion is the most important that ever was brought before the Obstetrical Society, or the medical men of Dublin. (Hear, hear). And now I ask you that intend to be or are doctors or surgeons of hospitals, do you think this question will stop here? If it has been proved that our midwifery hospitals are hot-beds of puerperal, what is to be provided for the medical attendance of the poor? It may be said that typhus has its habitation in the Meath

Hospital, where doctor after doctor dies. It might be said of the Richmond and Steevens' Hospitals that erysipelas is a constant lodger there. (Hear, hear). Do you think then this question will stop here? [Dr. Kennedy, certainly not]. Certainly not, Dr. Kennedy says, and therefore I say to you that you must be all up and doing. "There's a chiel amang ye takin' notes, and gude faith he'll prent them." Perhaps this might be interesting with reference to the terrible contagion of zymotic diseases among horned cattle. I asked my friend, Dr. Hugh Ferguson, to give me his opinion of them. He said—"I declare to God, Dr. Sawyer, all I would have to do is to throw the coat in which I had examined a diseased animal amongst a byre of cows to give them all puerperal." I said, "this is a topic that is engaging the attention of us all, and give me your opinion of it." He did so, and I will read it for you with your permission:—

"Veterinary Department of the Privy Council Office,
"Dublin Castle, *May* 15, 1869.

"Dear Dr. Sawyer,

"In reference to the subject of our conversation on yesterday evening relative to puerperal fever, to prevent any misunderstanding relative to my statement, I deem it desirable to give it to you in the form of a written communication in reply to your inquiries; and, in doing so, beg to impress on your mind that I confine my comments to such animals as are the legitimate objects of veterinary treatment, except in giving my opinion relative to the comparative frequency of the disease in the human subject and in the brute creation.

"Puerperal fever is of more frequent occurrence in the oxen tribe than among any other class of animals, mankind included. It exists to a much greater extent at present than it did in any spring of former years. Spring is the period of the year at which almost all herbivorous animals give birth to their sexual produce. It is the earliest season of the year at which the maternal herbivorous parent can get a sufficient quantity of succulent, milk-producing food to enable her to properly nourish her progeny, and at which the general temperature of the atmosphere is mild enough not to be inimical to the vitality of the newly born animal. The greatest number of quadruped births taking place in May and June, they are the months of the year in which puerperal fever is most frequent in Ireland among herbivorous animals. The disease is highly infectious, so much so that it can be carried even by the clothing of the attendants almost any distance from diseased animals to healthy ones. The malady although so highly infectious and contagious, frequently occurs totally independent of the influences of contagion or infection; but once generated, no matter from what cause, can be transmitted or conveyed to other parturient animals of the same kind, even through the medium of

inorganic matter, but more particularly by such animal products as are used for clothing purposes. It is a question yet to be decided whether the infectious principle of puerperal fever generated in one kind of animal can be transmitted, under any circumstances, to a parturient animal of a different kind. There are some diseases to which quadrupeds are subject, and which can be transmitted from them to the human subject in such a way that the malady is exactly the same in both. Scarlatina affords a marked example of this fact. Many believe that scarlatina never occurs except as a result of infection or contagion. My experience leads to quite a contrary conclusion. My attention was first directed to this point about twenty-five years ago. I was called, some miles from town, to visit a young horse which had not been off the same premises for nearly a year, and had not come in contact with any other animal, nor with any person affected with scarlatina, or who had been in any way, as far as could be ascertained, exposed to the influence of the malady. The animal, being a favourite with its owner, was sent into my hospital, then in Denzille-street. My hospital-groom had a child who was in the habit of going with its father round the hospital every morning. On one occasion the father and child were in the stable in which the horse was kept for upwards of an hour, while it was being whitewashed. Shortly afterwards the child 'sickened;' became affected with scarlatina, in a malignant form, and died. He never rallied, but rapidly sank. It is possible he might have got the disease from one of his own species; but I have seen several similar instances. Many medical authorities assert that glanders is communicable from the horse to the human being in exactly the same character as it affects the solipedal quadruped; but such is not the fact. The inoculation of the human subject with glanderous matter from the horse will, in the majority of instances, prove fatal; but the same symptoms, the same result, and the same *post mortem* appearance, in the majority of instances, follow inoculation with the morbid virus of other diseases besides glanders. There is a well known fact connected with the procreation of the species, which, were I a practiser of obstetric human medicine, I would submit for the information and consideration of the medical profession, and that fact is, that if, even from accident, a case of abortion occurs among a number of gravid animals kept together, the same abnormal occurrence, or repetition of it, will take place to incomparably a greater percentage of extent than that of the transmission of any disease by infection or contagion from one human being to others of the same species. This is a matter which I respectfully submit is worthy of consideration, not alone of the veterinary, but also of the more legitimate and scientific profession of human medicine.

Apologizing for thus intruding so long a communication on your patience, "I am, dear Sir, yours very faithfully,

"To Dr. Sawyer." "Hugh Ferguson, H.M.V.S.

This is the inference he wishes to convey as to its communicability from the lower animal to the human species. That is his point—whether he is right or wrong. It is after ten o'clock, and I declare I feel I have trespassed too much upon you, although I have not said half what I had contemplated bringing before you.

May 29.—The adjourned debate on Dr. Kennedy's paper was opened by Dr. Beatty who said :—

At the conclusion of the last meeting of this Society Dr. Kennedy complained that this discussion had not been carried on fairly, inasmuch as the speakers who have taken part in it had come down with long prepared essays, in reply to his paper. Now Dr. Kennedy should recollect that his is a very long, and most carefully prepared essay, upon which he must have bestowed very great pains. He should recollect that he has given the whole strength of his acute, and well stored mind, and brought the weight of his long and great experience to bear upon the most important question that was ever debated in this Society. He should recollect that in that able and elaborate essay he proposes nothing short of a revolution; and moreover that he occupied two nights in its delivery. I think, therefore, that it is scarcely fair of Dr. Kennedy to deny to others the same latitude which he assumed for himself. The reasoning, the statements, the startling conclusions, and the sweeping suggestions contained in that very remarkable paper, could not be satisfactorily scrutinized and replied to, without an amount of preparation and an extension of time unusual in debates on ordinary occasions in this Society. I trust, therefore, that Dr. Kennedy will perceive that it is the unusual importance of his communication that has necessitated the length of the replies. If in the observations which I am about to offer I transgress by occupying more time than to many may seem convenient, I must plead the same apology, and assure Dr. Kennedy and the Society that the subject is of such deep interest I found it impossible to condense my views into a smaller compass. The question before us has been so very ably handled by the distinguished speakers who have preceded me, and the assertions and suggestions contained in the paper under discussion having been so well combated, but little remains to be said by me on this the fourth night of discussion of this most important subject. Discussion is indeed not a correct phrase to apply to our recent proceedings, for all the speeches and arguments have been on one side, and in contravention of the paper, and not one voice has been raised in its defence. So much of statistics have been necessarily introduced into the addresses of previous speakers, and they have so well answered the purpose for which they were used that I shall not weary the Society by any allusion to that dry subject, further than to supplement the table of private practice in this city, given by Dr. M'Clintock at a former meeting. In the year 1853, I had the honour of

delivering an opening address from the chair of this Society, when I took occasion to allude to the private practice of my father, the late Dr. John Beatty. It may be in the recollection of some of the members present that I then stated that in 38½ years, ranging from 1793 to 1831 he had attended 5,616 private patients, of whom 13 died, of these 13 I find the following record.* In my own practice out of 2,064 patients 17 died from the causes mentioned in the annexed table,† and adding the two together we have a total of 7,680 cases with 30 deaths.

	Deaths from Accidents of Labour	Deaths from Puerperal Diseases	Deaths from Non-Puerperal Diseases	Total
*Dr. John Beatty, 5,616 cases,	2	9	2	13
†Dr. Thos. E. Beatty, 2,064 „	4	8	5	17
Total, 7,680 „	6	17	7	30

If Dr. M'Clintock will add these records to his tables of private practice and deduce the results in a similar manner, it will add to the importance of that part of his most able argument.

In the commencement of the paper now under consideration Dr. E. Kennedy says, his object is to investigate the true nature of and the laws which regulate the production of puerperal fever; and that the tenor and purport of his observations will be directed principally to prevention. These are in fact the material points of the great question under consideration, and to them I shall confine my observations on the present occasion.

In May, 1834, I attended a young healthy lady in her fourth labour. She resided three miles from Dublin, in a large house and demesne, in a healthy locality, and she had not been outside her own gate for a month before her confinement; but took exercise freely in the grounds. She was dead of metria on the fifth day after delivery. The child to which she had given birth was attacked with erysipelas, of which it died on the eighth day; and the monthly nurse who attended her, was also seized with erysipelas, and recovered after a severe struggle. Now what was the cause of this terrible tragedy? I will tell you. A very dear relative of mine had recently suffered amputation of the breast at the hands of the late Mr. A. Colles; I was, of course, assisting at the operation as well as at the daily dressing of the wound. Before that was healed the lady was seized with erysipelas which quickly spread over her whole body. During one of the daily visits I was summoned to my patient in the country to whom (having washed my hands) I was conveyed in an open vehicle and

without any outside clothing. I was not then as well aware of the impropriety of my conduct as I am now. I did not know I was carrying that lady's death-warrant from the chamber of the patient I had just left; neither for my own peace of mind did I know it for long after; not until accumulated facts recorded by others, and observed by myself convinced me of the true nature of metria and its too fruitful cause. Now if instead of going to that lady I had walked into a ward in the cleanest and best ventilated hospital in the world, and there attended a healthy woman in labour, the result would have been the same. I would have inoculated that patient with erysipelas which would have shown itself as erysipelatous inflammation of her uterus and peritoneum or pleura or pericardium, (for I have seen it begin in each) with its characteristic fever, to which the name of metria or puerperal fever would have been applied. Let me relate another and somewhat similar case. In 1855, I attended a lady in her first labour who was attacked with external erysipelas of the nates and thighs which soon extended and became of the gangrenous form of which she finally died. When she was at her worst, on paying my morning visit I missed the monthly nurse; and on inquiry I found that she had been sent for in the night to attend another lady to whom she had been engaged. I was shocked when I heard this, but it was too late to interfere; the mischief was already done. Three days afterwards I was asked to see a lady in consultation, and on arriving at the appointed hour the first person I met on the stairs was the nurse just alluded to. I made up my mind on the instant as to the nature of the case, and before I saw the patient I pronounced in my own mind, "fatal metria," and so it was; the lady was dead in forty-eight hours. That nurse had a severe attack of erysipelas afterwards.

A most important paper on this subject by Mr. Sidey, of Edinburgh, appeared in the *Edinburgh Medical and Surgical Journal* for 1839, a paper which seems not to have attracted that attention which its great value demanded. He gives a series of cases confirming the indentity of erysipelas and puerperal fever, to which I will briefly call your attention. Case I., Mrs. C., was delivered of her third child after an easy labour; she shivered on the second day, and died of puerperal fever on the seventh day. Her servant maid was attacked with erysipelas on the fourth day of her mistress's illness, which seizure ended fatally. Mrs. C.'s husband on the fourth day after her death was seized with fever and inflammatory sore throat, with deep dusky redness all over the fauces and small pustules over the uvula, great tenderness of the larynx, and great difficulty of deglutition. His recovery was doubtful for some time, but he ultimately got well. Case II., Mrs. M., seventh child, labour easy, died on the 6th day from puerperal fever.

Five cases of erysipelas among Mrs. M.'s friends happened during the week following her death. 1. Mrs. M.'s sister-in-law, who had assisted

her during her illness, died within eight days of great abdominal irritation. 2. Another sister-in-law got fever and erysipelatous sore throat, and recovered. 3. The mother-in-law got erysipelas of the face and head, followed by great debility, but recovered. 4. Mrs. M.'s son, a boy, was seized with erysipelas of the face. 5. Mrs. M.'s daughter, seven years of age, was seized with fever and erysipelatous sore throat, which continued some time. Case III., Mrs. J., had a quick pulse and hot dry skin during labour; she shivered on the night of the second day, and died on the sixth, of puerperal fever. Her infant, when eight days old was seized with erysipelas; the penis and scrotum became quite gangrenous, and hedied in a few days. Case IV., Mrs. H., had twins. On the second day she shivered and got smart fever, and was dead in six days. The child died in a few days with swollen bowels and oppression. Case V., Mrs. C. was seized on the second day after labour with fever, from which, after a long and severe struggle, she recovered. Her child, when eight days old, was attacked with fever, tympany of the belly, moaning, and vomiting, and died in thirty hours. The abdomen was found full of bloody flaky serum. Dr. Imlach, who assisted at the opening of the bodies of the third and fourth cases, was seized four days thereafter with a considerable degree of fever and erysipelatous inflammation of the internal fauces and throat, which left great debility.

Mr. Sidey then gives an instance showing how erysipelatous inflammation will give rise to puerperal fever:—"An out-day patient of the Lying-in Hospital was in the habit for some days of waiting upon a relation who was ill with an erysipelatous inflammation of the knee-joint. Upon her return to the hospital she took charge of a new-born child. Two days afterwards the infant was seized with erysipelas of the abdomen, of which it died. On the third or fourth day the mother was attacked with puerperal fever, and she, with some others, sunk under it. The hospital was quite free from the fever previous to the appearance of erysipelas on the child." Dr. Alison, after this paper of Mr. Sidey's was read, mentioned a case which occurred to him in the Royal Infirmary. "A young woman who had an erysipelatous inflammation of the mamma, miscarried about the third month, and very soon afterwards the inflammation left the breasts; the abdominal cavity was affected, and all the symptoms of puerperal fever came on. In the course of a few days she died, and upon opening the abdominal cavity it was found filled with a bloody flaky serum, without any adhesion of the internal parts." A condition very unlike that which would have been revealed if she had died of ordinary sthenic peritonitis.

It has been long known that when erysipelas is prevalent puerperal fever is abundant; all writers agree upon that point, and in most hospital returns particular stress is laid upon it. In my own report

of the South Eastern Hospital I mention three outbursts of puerperal fever, but observe that on each occasion erysipelas was very prevalent in the city. In the last statistics that has been published—that in the last volume of the *Transactions of the Obstetrical Society of London*—an account is given of the very fearful mortality in the obstetric wards of King's College Hospital, called the Nightingale. It is there stated that in the last year of the maintenance of that department, out of 125 women delivered in 1867, 9 died, or 1 in 13·8. During the six years of its continuance the mortality was 1 in 28·9—a mortality, as the reporter says, larger than that of all the other hospitals in the world, and clearly to be ascribed to the location of lying-in women in a general medical and surgical hospital. Mr. Rowling says also that he has no doubt that the outbreak of erysipelas and pyemia which occasionally occurred in the surgical wards was the signal for the occurrence of puerperal fever in the Nightingale ward. During this time an interesting fact occurred showing the connexion between erysipelas and puerperal fever. The child of the last patient who was delivered by the forceps, was slightly marked by the instrument behind the left ear. From this point erysipelas started, spreading over the face, and the child died. The mother having also died on the fifth day of puerperal fever. I could multiply such evidence to a wearisome extent, but I will turn to the negative side of the question, and call to your recollection the letter read at one of our last meetings by Dr. Denham from Dr. Johnston, of Kilkenny. In that town and surrounding districts he says puerperal fever is unknown; and he gives a satisfactory reason for the immunity of that El Dorado, when he tells us that erysipelas is also unknown. Here, then, we have abundant evidence that when erysipelas is prevalent so is puerperal fever, and that when erysipelas is absent puerperal fever does not exist. Here, again, we have abundant evidence of the identity—I don't say the resemblance, but the identity—of the poison in the two conditions, the effects merely differing according to the class of organs obnoxious to its assault. When the epidemic influence of erysipelas is abroad a man with any kind of wound or scratch may be seized by the disease and may be killed by it. A woman in the next house may have undergone the serious operation of parturition, in which a certain amount of violence, and often cracks and tears of the internal and external genital organs are inflicted; the same epidemic influence that killed the man is also round and about the woman, and cannot be shut out. What it is we do not know; the nature or essence of it we know nothing about, but we do know that the woman may be and often is killed by a disease which would not have assailed her if that epidemic influence were absent. Twenty men and twenty women in the same locality may be similarly affected by erysipelas without the slightest intercourse between the

houses by friends or medical attendants; and thus the true *epidemic* nature of the disease is exhibited. But if incautious communication takes place between man, woman, or child affected with external erysipelas and a healthy parturient woman, then the *contagious* nature of the disease is shown; the injured organs of the woman are the weak points seized upon, and from the well-known serious consequence of any kind of inflammation affecting these organs, this asthenic inflammation too often ends fatally.

Now we are all familiar with zymotic diseases of different kinds, and we all know something of the laws by which they are governed. We know that it seldom happens that two of them are prevalent at the same time; but we know most certainly that they are not convertible the one into the other. Who ever heard of scarlatina in one patient engendering measles or small-pox in another by contagion? It is contrary to all we know of diseases and of contagion to harbour a belief in its possibility. But while we all acknowledge the truth of this axiom in general, we are every day denying it in the case of what is called puerperal fever. That is and has been considered a distinct specific zymotic disease, and so has erysipelas; and yet we have incontrovertible evidence that they always coexist as epidemics, and that they are convertible in a remarkable degree—nay more, we know that, as in the case of Kilkenny, where the one is unknown the other never appears. It is of the highest importance that a clear line of distinction should be drawn between what is called puerperal fever and common acute inflammation of the peritoneum and uterus. It is evident that the latter disease has been frequently taken for the former, and it is clear that from this circumstance arise, in a great measure, the different views entertained on the nature of puerperal fever, the difference in treatment, and the great difference in success in one practitioner over his fellow, both of whom have been treating different diseases. And to show the want of unanimity among practitioners upon this disease, we have only to remember the varieties or opposite modes of treatment recommended by each, and every one more confident than another that his is the most successful.

What do I deduce from all that I have said? I think the conclusion to be drawn is perfectly clear, reasoning upon common principles. I must agree with the opinion pronounced thirty years ago by Mr. Sidey in the paper alluded to. He says:—"I think it a disease quite distinct from common acute inflammation of the peritoneum, uterus, and its appendages. Combining the symptoms, the result in practice, the state of the parts disclosed by examination after death, and the facts of the above-cited cases, to which many others could be added, I think the inference to be drawn is that puerperal fever is merely an erysipelatous inflammation of the peritoneal covering of the pelvic region, or other serous

membranes, extending as the disease advances over the abdominal cavity and the body of the uterus, accompanied by true erysipelatous fever."

I am forced, therefore, to the conclusion that what is called puerperal fever is not a distinct zymotic disease, but is only a form of erysipelas exhibited in parturient women. I am happy to find that this opinion is entertained by Dr. M'Clintock, who, in the admirable address delivered at a former meeting, made use of the following words:—"But after all, though it is convenient to distinguish erysipelas and zymotic metria as two different diseases, still there is little room to doubt that they are one and the same disease." Now, that erysipelas is not only an epidemic but a highly contagious disease no one at the present day can possibly doubt. Is it not an idle waste of time to occupy ourselves in vain attempts to discover the nature of, and the origin of, and the best means of preventing what is called metria, as if it was a distinct and separate disease? Instead of such a fruitless pursuit let us turn our whole attention to the investigation of erysipelas, and the laws by which its dissemination is regulated. These, by diligent attention and observation of facts, we may arrive at, but we make little advance towards any useful knowledge whereby to combat disease by such speculations as we find in the beginning of Dr. E. Kennedy's paper. Take one passage as an example, viz.:—"Should the principle of isomerism, which has of late attracted much notice with our chemists and professors of physical science, come to be established, the difficulties in adopting the idea of a common morbific poison would be lessened. As then the poisonous principle, which we might denominate *zymotocene*, would come to be classed as an original polyatomic molecule or principle, and would fall strictly within Dumas's definition of what he terms *polymorphism*, namely—One of those variations in the arrangements of integral molecules of a body which influence its physical properties, either temporarily or permanently. There is nothing at all unreasonable, therefore, in supposing that, as in the case of polymeric or polymorphous hydrocarbons, we may also have the germs of different disease produced by polymeric combinations in the same elements." Now I ask every one present is he one bit the wiser when he has read that passage?

The principal argument upon which Dr. Kennedy founds his hostility to lying-in hospitals is that metria is generated and spreads by the congregating of parturient women in the same building. "The local cause," he says, "approaches more nearly to a *constant* quantity in the wards of a crowded lying-in hospital, whereas it is only an *occasional* quantity in the houses of the affluent; and the only influence exercised in its production by the atmosphere is that in certain states of the atmosphere, the constant and occasional quantities become *more operative* or *active* in generating or propagating this dreadful malady—a malady thus zymotic in its type and origin produced by a poison emanating from parturients;

more active in proportion to the concentration of their excretions or exhalations, and consequently in proportion to their number cohabiting in a given number of feet of atmospheric space, but not requiring more than one parturient female to generate it, when the poison she herself has generated may, as in the case of blood-poisoning, be reabsorbed into her own system, and self-contamination then as certainly strike her down as if a crowded, ill-ventilated lying-in ward was the generating medium."

If that be the case why, I ask, is it not always present in such institutions? which it is not. And why is it not always most intense when the greatest numbers are collected together? which is notoriously not the case. Why have there been long intervals of years, sometimes when the largest number of women have been confined, during which no metria has appeared? which is quite true. If the collecting together of lying-in women be the cause, and that cause remains constant, the effects should also be constant. There is no reason why the laws of nature should be different in this from every other instance in which known causes are followed by known effects. We know that fermentation is generated in a dung-heap by the collection of the materials of which it is composed, and that heat is evolved and continues without any interruption as long as fresh materials are added to it. In this case we find no capricious interruption of the process for months or years, but in obedience to nature's laws the cause continues to produce its well-known effects, and fermentation goes on uninterruptedly. Human diseases are governed by nature's laws, and we have no right to invent exceptions to suit our own theories. We are not obliged to resort to such an unphilosophical explanation of the appearance and disappearance of metria in hospitals. We know that when erysipelas is epidemic metria is sure to appear, and when erysipelas subsides so will metria, if separation for a time be enforced, and the disease is not perpetuated by contagion. For these reasons I must withhold my assent to the doctrine of the spontaneous generation of puerperal fever. It may seize a woman in hospital as it may the highest lady in the land, when the epidemic influence is abroad; or it may be introduced by contagion into an hospital as well as into the chamber of a duchess, and when once there all within its range are in danger.

This brings me to the consideration of the second subject proposed by Dr. Kennedy for discussion—namely, how to prevent puerperal fever in hospitals.

I have expressed my belief that there are two modes by which metria or erysipelas obtains an entrance to the chambers of lying-in women, whether in private houses or hospitals, viz., epidemic influence and contagion. If the disease is epidemic it will make its way into the large hospital or isolated hut as surely as it does into surgical hospitals

or private houses; and the woman who has recently undergone the operation of parturition is, to say the very least, as liable to its assault as any patient, male or female, who has been the subject of a wound, either accidental or intentional, by the surgeon. We know that during the prevalence of such an epidemic no surgeon will perform any operation that he can possibly avoid. But the pregnant woman cannot postpone her operation. If we had the power of suspending nature's operation as the surgeon has to withhold his knife until "the pestilence that walketh in darkness" has passed over the land and vanished, the death-rate from metria would soon dwindle to a very low figure. But although we are powerless to shut out this first mode of infection, we are not so in the second. Contagion can be met, and the door can be shut in its face. To see how that can be done let us inquire who attend the patients in a lying-in hospital? A class of pupils chiefly composed of medical students. Again, let us inquire how these gentlemen are employed during the day when out of the lying-in hospital? They have their medical and surgical hospital to attend in the morning, when they are of course exposed to any malaria prevalent in the wards; and if they are dressers in the hospital and have to come in contact with wounds and ulcers, and compound fractures, many of them perhaps surrounded with erysipelatous inflammation, they necessarily carry away a certain amount of infection. If they pass from the wards to the dead-house, and there take part in *post mortem* investigation, passing a considerable time with their hands buried in a diseased and putrifying abdomen; if from that they proceed to the dissecting-room and there pass hours in the contaminated atmosphere of such an apartment, and in direct contact with flesh in every stage of decomposition; if such be the ordinary daily course of a medical student, I ask is such a person fit to walk into the wards of a lying-in hospital and attend a woman in labour in the evening? All who hear me know that this is not an overdrawn case. But when this source of danger is multiplied by six, eight, or ten pupils who have pursued the same course during the day, and frequent the same lying-in hospital during the evening and night, and then with laudable zeal to learn midwifery, examine every woman that may be in labour; if this takes place during a run of erysipelas in the surgical hospitals and while epidemic influence is abroad, and thus both doors are opened for the entrance of disease, how can we be surprised that fatal erysipelas so often makes its appearance. The wonder is not that some women die but that the number is not vastly greater. Now this is a danger against which no isolation of the patient in huts can protect her. My unfortunate patient, with whose case I opened these remarks, was in a most perfect state of isolation, nevertheless she fell a victim to erysipelas conveyed to her by her attendant; and every patient in any series of isolated huts that could be constructed would be equally

exposed to contamination when their attendants were composed of a class of pupils such as I have described.

That metria is more prevalent now than it was forty years ago is a fact too well known; and why is it so? I think there are two causes in operation sufficient to produce this result. The first is the change in the type of disease which has taken place in this country in late times. All diseases have assumed a typhoid or asthenic form now-a-days, which was rare and the exception in the early part of this century. The kind of disease which then prevailed is, I may say, extinct. The true inflammatory fever—the synocha of Cullen—is now unknown; the pupils of the present day know as little about it as they do of phlebotomy and the use of the lancet. Inflammations which then appeared with full bounding pulse, flushed face, and burning skin, no longer present themselves, actually courting depletion; the same diseases are now all of the low typhoid type, in which depletion is inadmissible and stimulants are demanded. Hence it is that inflammations in lying-in women generally assume the erysipelatous instead of the phlegmonous form, and the results are more generally fatal.

The second cause is the difference in the class of midwifery students. It is only of late years that practical midwifery has been made a compulsory part of the curriculum for medical and surgical degrees; formerly, when I was a student, it was not so; and when I was a Fellow of the College of Surgeons and an M.D., I had never seen a lying-in woman. The practice then was not to attend the lying-in hospital until after all other school and hospital attendance was over, and many of the class were already graduates. You will readily perceive how a class thus constituted was less likely to carry erysipelas into hospital than are the classes of the present day. Great stress has been laid by Dr. Kennedy on the immunity from disease enjoyed by the maternity hospitals in New Ross, Waterford, and Limerick; but before we allow the superiority claimed for these institutions because they are small, we should ascertain who were the attendants upon the patients delivered therein. Now, we all know that there is no such class of pupils in those localities as I have just described; the women were most probably attended by resident midwives, with the assistance of a resident physician under the direction of the visiting medical officer; and with this absence of the means of daily importation of disease from without, the parallelism of these hospitals with the larger, where a large class of pupils are the attendants, is destroyed, and the argument founded on their salubrity ceases to be of much value. Another and probably not unimportant cause may be assigned—I allude to the more frequent *post mortem* examinations that have been made of late years. Pathology, as is well known, has been cultivated with considerable ardour during the last half century, and this laudable desire to accumulate facts and extend the

boundaries of knowledge was partaken of to a considerable extent by the masters of the Rotunda, and by none with more vigour and industry than Dr. Kennedy. There was no Pathological Society in former times to which specimens could be brought by those interested in science, or ambitious of obtaining a reputation for observation and industry; there was no temptation to pry into every cavity in every patient that died in any hospital, so forcibly described by Dr. Arthur Jacob in one of his allusions to the Pathological Society of this city, which he never loved. Speaking of the manner in which subjects for dissection were mutilated before they were suffered to leave the hospitals, he described the investigators as "actually trawling for specimens." The Pathological Society of Dublin had been just recently established when Dr. Kennedy was Master of the Rotunda Hospital; and I have a lively recollection of the number and value of the specimens which he produced at our meetings. If Dr. Kennedy looks back to those days, as I have done, it is not improbable that he may attribute some of the serious mortality that occurred in the hospital during his mastership to the zeal with which he and his assistants cultivated pathological knowledge in the dead-house.

I have said that contagion can be met at the threshold of the hospital and forbidden to enter. I said so in a lecture delivered thirty years ago in the Medical School in Park-street. I then expressed my firm conviction that midwifery ought not to be studied practically until after the pupil had obtained his diploma, and that during the six months of his attendance he should attend no other hospital. I argued that to follow this course would be also most beneficial to the student; that with his mind unencumbered by other pursuits he could then give his whole time and attention to the acquirement of a real and sound knowledge of this branch of medicine, and not leave the institution with the superficial and crude ideas of its importance with which, under the present system of hurry, they are obliged to be content. That opinion has been strengthened by time; and, like Dr. Kennedy's thirty years of incubation, I now produce my egg fully hatched.

Midwifery must be learned, and can only be learned by practical experience; but it is not to be learned at the expense of the lives of the patients. I would allow no pupil to attend to any maternity institution while he was in attendance on any other hospital or dissecting-room. Theoretical lectures on medicine and surgery, and all the other branches not bringing him in contact with dead bodies or disease he might attend, but a strict rule enforcing absence from the sources of infection should be at once instituted and rigidly enforced. This may cause a little delay and inconvenience to the pupil, but is that to be put in comparison with the preservation of the lives of the patients? Again, I would have no person in the institution, whether pupil, assistant, or master, take part in *post mortem* examinations; but a separate officer, who would never enter the

wards, should be attached to each institution, upon whom the duty of necroscopist should devolve. I firmly believe that the adoption of these suggestions would cut off the second mode of contamination to which I have asked your attention—viz., contagion, and that the assaults of metria would be confined to the first or epidemic influence, or the unhappy reception of a patient in whom disease has already commenced. Against epidemic influence no human barrier can be set up, either in palace, hospital, house, or hut, except ventilation and scrupulous cleanliness, both of which are rigidly enforced in the institutions of this city. I would finally recommend a good generous dietary to be established in all maternities, as a sure means of enabling patients to resist the demon metria. The adoption of these suggestions would, I believe, satisfactorily answer Dr. Kennedy's second proposition, namely—the prevention of puerperal fever, and would remove any excuse for the revolutionary process and plan by which he proposes to destroy a time-honoured institution which has been so long the boast and pride of every Irishman.

Dr. Stokes said :—Mr. President, I have to offer my apologies to you and the members of the Society for intruding a very few observations upon this meeting, but I think it may not be unwelcome to this Society to hear some remarks from one who is in another walk of the profession from those distinguished gentlemen who have spoken upon this question. I may observe, as a preliminary, that I do not entirely agree with my friend Dr. Beatty, and other speakers here, upon the absolutely defined character of contagious disease—that is to say, I do not believe that a poison, which, as far as we know, is the same, will necessarily produce the same disease. I think that is an assumption which is not yet perfectly established. We must recollect that in the production of a disease from the introduction of a poison there are two elements—one, the poison, and the other the state of receptivity of the constitution into which that poison is introduced, and therefore it is perfectly possible that a poison of one disease may produce the appearance of another. I shall not enter into the evidence of this ; it would be out of place here. There is no doubt whatever of the existence of mule diseases—diseases that are between scarlatina and measles, typhoid and typhus, and between measles and small-pox—and all this points to the operation of the two elements in the production of any particular disease through contagion. I feel that I am intruding. I know very little—indeed, I may say, nothing—of obstetrical science. I never attended a woman in her delivery ; I never was in the room with a woman during delivery ; and I look upon that as rather increasing the value of anything I may say here to-night, because I look at the subject from a different point of view. Now it appears to me that this great discussion which has lasted so many nights, has much more the character of a

debate than of an argument. I think it has been characterized by the controverting of the opinions of one individual, and not by a general argument upon what was the more important question—the real nature of metria; for the opinions of Dr. Kennedy, whatever they may be, must more or less be determined by our views of the nature of metria. I think Dr. Kennedy will agree with me in that, and therefore I think that, as ancillary to the question of the change of the arrangement of hospitals, a very important preliminary investigation should be made, if possible, into the real nature of metria, as near as we can get at it. Now the great question which has been debated is not the nature of metria, except I think in the observations of Dr. Denham and Dr. Beatty. The question has been less the nature of metria than this; whether the development and the fatality of metria are in proportion to the number of parturient women congregated together. That is the issue which we have to try here. Now if it be true that the fatality and prevalence of metria are directly as the number of parturient women congregated under one roof, we must believe with Dr. Kennedy—a view which I must say must be received with the greatest respect, looking to his position, and his character, and his experience—that the parturient woman has in herself a power of generating a poison which infects herself, which produces a similar process by contagion in her neighbours, causing a disease which follows the law of what it is the fashion to call zymotic disease—a term that I have a great objection to—which then acquires a great impulse from overcrowding. I think I have fairly stated the issue before us. I think it is still an open question whether the parturient woman, as such, is ever a generator of a specific poison. That is a very important question, and, as I said before, it appears to be the opinion of Dr. Kennedy, and we must receive it with great respect—or, is it that under the circumstances of parturition, the woman is more liable to epidemic influences—whether she be in hospital or not. Now to hold that a natural process, such as parturition, should be of necessity coupled with a tendency to develope a specific and fatal poison seems to be a startling proposition, when we remember that on that natural process the existence of man upon the earth depends. If it be proved to be so, that coincident with the natural process of the production of life, there is a natural production of a state of death, I think we may safely say in the words of Professor Haughton, that nature is not only a step-mother but a cruel step-mother. I hold it at least to be a very startling proposition. I am not in a position to deny it, but I think the meeting will agree with me in holding that the verdict which should be taken on the case, is the Scotch one of "Not proven." The population of the earth may be roughly stated at 800 millions. We may also roughly state that one-half of that number consists of women. If this be so, the number of cases in which this poison has been generated

is so much less than those where it has not, that the rarity of the occurrence is almost conclusive against the doctrine. Now in reference to the doctrine of the parturient woman producing a specific poison, which is followed by the so-called zymotic disease, we may suppose two women delivered in the same ward, within six hours, and attacked with rigors within an hour of one another. We might ask which of these women was the generator; or, we might ask were not both of them the subjects of an epidemic influence. There is a rule in philosophizing that where there are two methods of explaining a certain phenomenon, we should accept that which is the most probable.

Now let me refer to Dr. Denham's remarkable statement with respect to the occurrence of puerperal fever in undelivered women. Is simple pregnancy the causes or the source of the development of a malignant poison? Who would believe that? The condition of a parturient woman has been compared to that of a person after a surgical operation. The analogy is by no means perfect. In one case the patient is the subject of a natural process, a process which the Almighty intended to be perfectly consistent with health and with life. In the other case, the patient who is subjected to a great surgical operation is the subject of an entirely different process. The analogy may be good in a mechanical, but it certainly fails in a physiological point of view. If the supposed poison be generated in the uterine passages, I would ask how it comes that every possible form of abnormal discharge may occur in the newly delivered woman without being followed by puerperal fever? I think I may appeal to the practical obstetricians in this Society in confirmation of this statement. Then with respect to the question of the communication of puerperal disease by men who have been employed in dissection or in attendance upon surgical cases, that is another question. But I think that a little too much has been made of that too; because if the contact of the hands of medical students with parturient women was so apt to produce puerperal fever—and mind I don't deny that it is so—it has been asked by Dr. Head, why in the intervening times these men do not produce erysipelas in surgical hospitals? But if all this be so, what, I would ask, becomes of the theory that it is the woman herself that produces the specific disease? It goes to the ground. If this so-called zymotic disease can be produced by the contact of the hand of the student who is fresh from the dissection of a non-puerperal subject, what becomes of the theory of the spontaneous production of the special poison producing a special zymotic disease by the puerperal woman herself? The close relation between erysipelas and childbed fever is now thoroughly recognized. Dr. Beatty's admirable paper to-night has, I think, carried conviction to the minds of every one who heard it on that point. It is also greatly advocated by the latest and most eminent of the French authors, namely, Trousseau. He insists upon the fact that the erysipelas of the

new-born infant is closely related to puerperal fever. He goes further, and says that the new-born infant who dies of erysipelas dies of puerperal fever. He shows, or endeavours to show, that the new-born infant is more or less in the same condition of receptivity of disease as the lately delivered woman, that is, very much in the condition of the layer of a plant—that it has much of its mother's life still in it—that it is subsisting upon its mother's blood,—and therefore whatever disease the mother is subject to, either by external or epidemic influences, so also is the new-born child. Trousseau attributes the frequency of erysipelas in the child to a condition somewhat analogous to the condition of the parts of the uterus after the separation of the placenta, the condition being the sloughing of the umbilical cord. But it is a fact that the erysipelas of the new-born child has been observed to occur before the process of the sloughing of the umbilical cord. Now I shall not occupy your time more than a few minutes. But I may observe on two *desiderata* in this matter; one is the statistics of erysipelas in new-born children who have been born of mothers who have had or who have died of puerperal fever; and the other is the statistics of erysipelas in new-born children whose mothers have not had puerperal fever. I think my friend, Dr. Sinclair, will be able to supply the meeting with some most important facts upon this subject. I myself have not the slightest doubt that contagion is less a cause of the spreading of puerperal fever in hospitals than epidemic influences. What these epidemic influences are as yet no man can say. They are unanalysable in the present state of our knowledge. Humboldt in his "Cosmos" speaks of this subject, and states that the laws of epidemic diseases are among the most obscure subjects of human investigation. And he states further that he is at a loss to suggest any possible solution of them until the laws of atmospheric and terrestrial magnetism are perfectly worked out. One of the most remarkable instances with which I am acquainted in favour of the epidemic influences in producing puerperal fever, as distinguished from contagion, in lying-in hospitals, is the great fact that women are simultaneously affected by puerperal fever in the hospitals where they have been entirely separated, and where even the attendants have not been the same.

Much stress has been laid upon the arguments from a comparison of the mortality of the cases treated in the great lying-in hospitals, and that of patients treated in their own houses. The reliability of the returns of deaths to the Registrar-General, as far as puerperal fever is concerned, is worth very little. I can state that from long experience. I state it strongly. Now, I think it hardly fair to compare these statistics with those of a regular hospital, and it is clear that to justify any organic change in the system and in the arrangements of the hospitals based upon these comparative statistics would be utterly objectionable. It also seems to me that we are very deficient in accurate statistics as to

the mortality from puerperal fever in hospitals and in private practice. Well, now, I will tell you my experience upon that subject. I have been a practising physician in this city for more than forty years. I have been repeatedly called in consultation to cases of puerperal fever in the better class of life, and I am sorry to say that the result is, that in these forty years I have never seen one case of puerperal fever in private practice that was not fatal—not one; and yet these cases occurred in patients previously in perfect health, enjoying all the circumstances that would promote general health: patients isolated, living in large apartments, well ventilated, and scrupulously clean; and the patients carefully watched. And I shall never forget the observation of one of the most eminent accoucheurs of Dublin made to me many years ago, when he said, "I would as soon be called to a case of hydrophobia in private practice as to one of puerperal fever." Now, if the mortality in private practice, with all these favourable circumstances, be as great, or greater, which I believe it to be, than in the lying-in hospitals, what becomes of the argument that the fatality is in proportion to the number congregated under one roof? That there is a close analogy between puerperal fever and pyemia no one can doubt, and we know that there are epidemics of purulent disease. These have occurred in our own time. Dr. Ephraim M'Dowell has described an epidemic of pyemic rheumatism, where the joints became filled with pus, and where absorption did not take place, and the patient died with all the symptoms of diffuse inflammation, it might be said, the symptoms of puerperal fever. About 1836 we had an epidemic in Dublin, when every hospital presented cases of abscess of the liver—a disease previously unknown, I may say, and that lasted for about a year and a half; and during the last winter we have had an epidemic of rheumatic fever marked with great æsthenia, and, in a great number of cases, with a pyemic condition, and with great deposits of pus in the cavities of every joint of the body, and this constitutional condition tends to explain the mortality of puerperal fever, and the failure of all attempts to treat it as a local disease. Now, I conclude. When we speak of puerperal fever we must avoid all hair-splitting distinctions which exhibit a want of the *mens medica.* We must not talk of strong lines of distinction between puerperal fever and uterine phlebitis, and diffuse peritonitis, and so on—and again between primary and secondary local disease. Between local and essential affections there are plenty of distinctions laid down in books, but the physician who looks nature in the face will know what value to set upon them.

Dr. Murney said that Dr. Stokes' opinion must be received with the greatest respect. He should like to know at what time Dr. Stokes was called in to see those cases which were all fatal, where he did not get an opportunity of exercising his great judgment and experience.

Dr. Stokes.—I may say that I was called in at all stages, and always with the assistance of the most eminent medical men, both obstetrical and otherwise.

The President asked Dr. Sinclair could he supply the association with the information referred to by Dr. Stokes.

Dr. Sinclair said he was unable to supply the information that night.

Dr. Stokes.—In the few observations I made I omitted to state one fact, which will be of considerable interest in the solution of this question. It is a report which I have received from a former pupil of mine, a very distinguished man indeed, who has been for fourteen years the principal surgeon at Ballarat. I asked him about the diseases there, and he said the people were remarkably healthy. I said, "What diseases have you? Have you any fever?" He said, "No, we have no fever. There has been no case of typhus in Ballarat, with a population of 40,000, for the last fourteen years"—which is a remarkable fact of itself. "What diseases have you?" "We have two," he said, "and they are the two principal diseases of the settlement. These are erysipelas and puerperal fever." I said, "Have you got lying-in hospitals?" "No," he said, "nothing of the kind; all these cases occur in women who live in isolated houses or in cottages." I said, "What about the mortality?" "Oh, indeed, the less said upon that subject the better. I think I had one case of recovery, and I believe I was called in consultation to a second."

Dr. Henry Kennedy, on rising, said:—Were it not that different minds view this important subject in different ways, I would not venture to address this large meeting. With this feeling I shall endeavour to avoid ground already gone over; and here I would state my impression that an extreme caution should be exercised in arriving at any conclusions tending to alter existing arrangements, or allowing to emanate from such a meeting even the impression that our lying-in charities were about to be changed. For it is not a point but a great principle which is under discussion; and if the views of those who differ from me should prevail it would be not only our lying-in charities should be altered,—some would say revolutionized—but also our medical and surgical hospitals; and further still, even charities which do not come under the head of medical—nay, even our boarding schools must be done away; and as the principle would hold wherever civilization itself exists—it would necessarily entail an amount of expense which few countries are prepared for, and would afford a series of figures which, I promise, would be more tangible and striking than any which statistics give. As I give the author all credit for the best intentions, I might naturally wish to be able to view this momentous question from the same happy point he has

reached; I might wish to see the zymotic diseases made preventive in the rigid way in which he has put it; or, I might strive to flatter myself with the hope that the simple expedient of cottage hospitals was to banish from amongst us the dread disease puerperal fever. Sir, I have no such wishes nor hopes. I believe them to be fallacious, and founded neither on experience or facts when fairly interpreted.[a] In most of what has fallen from the author about the frequent occurrence of metria—of its great mortality, of its co-existence with other zymotics—and the fact of the domestic animals being often attacked with disease at the same time, I entirely concur. But I submit, with all respect, that these are not all the facts which ought to be considered. They give a *primâ facie* case in favour of the author's views. It is necessary, however, here to advert to other points. Epidemics have existed from the earliest periods of recorded time, and prevailed in every conceivable state of things; in summer and winter, in wet and dry seasons, famine and plenty, on the dry land and the ocean. In fact, it would not be possible to lay down any circumstance at all pertaining to them during which they have not prevailed. The law regarding them would seem to be as widespread as that of gravitation itself; and no doubt of it the Deity has, for His own wise purposes, ordained that it should be so; and I believe it will not end till time itself is no more. But again—and this is a very important point—they are not confined to man and the domestic animals, as our author's remarks might imply. On the contrary, all living nature is, in turn, attacked. The birds in the air and the fishes in the sea, and all the denizens of the earth are subject to this influence; and not only these, but the vegetable world is subject to the same power, and exhibits the results in a way which it is not possible to overlook. On this particular part of my subject I might easily dilate, but time forbids. Enough has, however, been stated to show the universality of this agency, and to lead to the conclusion that overcrowding and contagion play but a very insignificant part, if any, in the spread of epidemic disease. On these points, but particularly the latter, I entirely concur in what has fallen to-night from Dr. Stokes. The epidemic tendency, whatever it be, existed when there were no hospitals and no overcrowding; and as it exists still, it appears to me very unphilosophical to attribute it to these causes. And speaking of overcrowding reminds me that nothing of the kind, as far as I am aware, exists in the Rotunda Hospital; as, indeed, was stated by Dr. Johnston when first opening this debate. In contagion itself, as a means of spreading disease, I myself have very little faith; and I may take this opportunity, as bearing on the subject, of stating a fact which I brought some time

[a] Hence I, for one, must acquit the author of all acts of either omission or commission with which he has charged himself for not having brought the question sooner before the profession.

back before the Society. In the course of some twenty years pregnant women, to the number of nearly seventy, were admitted into the Cork-street Hospital, labouring under fever. Either abortion, miscarriage, or delivery, at the full time, occurred in all, while they were still ill of fever; and yet in not a single instance did I see any approach to puerperal fever follow. But some one will ask here:—"If contagion be not the cause, how do you explain the occurrence of several cases at the same time?" Very simply; and this I believe to be a very important part of my subject; and I wish I had time to enter more fully into it. I believe that the health of our poor, especially in large cities,[a] is almost always below par; and this state, it is well known, must predispose to disease. Hence, when the epidemic tendency prevails, parturient women are just the parties to be affected by it. The wonder, probably, is they do not suffer more; for parturition necessarily engenders a state fitting them for the invasion of disease. Why is it, I would ask, that a scratch which proves harmless in one person, will cause erysipelas, or gout, or pyemia in another? Surely, there can be no question here; it is because the system is in an unhealthy state at the time, and the scratch merely acts as a spark to light up a fire already prepared. So I take it to be with parturient women, and the fever engendered in them appears to me to be of just such a character as might be expected; for the analogy between an amputated thigh and the inner surface of the uterus seems to me to be complete—a point which has been just alluded to by Dr. Stokes. But some one will here remark, "All are not attacked;" and this is exactly what I would expect; for all are not in the same state of health at their confinement, and the risk is in proportion to this state. The nearer they are to health, by so much is the risk lessened, and *vice versâ;* for I hold that a really healthy woman will not take metria. On the other hand, I believe an unhealthy female is quite capable of engendering the disease within herself; and on this point I must venture to differ from Dr. Stokes, and I think I could prove it did time permit. These remarks naturally lead on to the question: "Are matters, then, to remain as they are? or can anything be done to lessen the mortality of the disease?" I believe much, but in a very different direction from anything which has been yet advanced. What I am about to say I would wish to be considered merely as suggestions—suggestions, however, which appear to me to have common sense and sound principles of physiology in their favour. What I would advise, then, is, that the health of the women who were about to be confined should be closely inquired into; and as the great majority of

[a] It was, I must say, with some surprise I observed that the health of pregnant women in large cities was compared with the same class in towns or villages. There must surely be a mistake here, and one into which I observe Sir James Simpson has also fallen.

those confined in the Rotunda apply previously for a ticket, I would then have the inquiry instituted, and a sufficient staff appointed for this express purpose. In sickly seasons, too, this might be carried still further; and I consider that those in authority would then be fully justified in having a house-to-house inquiry, just as was done, and with such effect, during the cholera epidemic.[a] This is not the place to enter into the details of what should be done. The rules, indeed, are so obvious as to strike the most ordinary observer. Only render the health of the women good, or tolerably good, and I have the strongest conviction they will pass through their confinements with safety to themselves and no risks to those about them. It was no doubt with the same feeling that my friend Dr. Denham suggested that the building of new dwellings for the poor would be followed by a diminution of the dread disease. The idea is excellent in its way; but till it can be accomplished the plan suggested by myself should be tried. It is feasible, and could be carried out with comparative ease, and at a very small expense.

In conclusion, then, when I consider that the epidemic influence is and must ever be amongst us; when I think of our noble charities of every kind, and the incalculable benefits they have conferred, when compared with their drawbacks; when I recollect they are human institutions, and therefore imperfect; and lastly, when it appears to me possible to meet these imperfections in a great degree, and at a nominal cost; I say, when I consider all these points I am forced to arrive at the conclusion that our charitable institutions should be left as they are, and not remodelled in any way.

Dr. John A. Byrne said he agreed with those who were of opinion that Dr. E. Kennedy was entitled to the best thanks of the Society for the papers which he had read. Nay, he would go further and say that the whole medical profession of the country and the public at large were indebted to him, as his paper had been the means of inducing a discussion upon a topic of such overwhelming interest as the prevention of metria or puerperal fever.

It is a curious fact that although many valuable contributions to the history, pathology, and etiology of this disease have emanated from some of our distinguished men here (a circumstance not to be wondered at considering the ample opportunities which have been, I may say, almost constantly and unfortunately afforded of studying this disease in this city), yet this

[a] In connexion with this part of the subject there are two facts worthy of note. The first was noticed by some of the speakers before me, particularly by Dr. Denham, and is most significant—viz., that it is not by any means uncommon for women to show signs of distinct illness before their confinement. No matter in what aspect we look at this fact it is all-important. The second is, that however severe the epidemic is some always escape—and these are the healthy.

is the first time that the matter has been formally brought before the notice of the profession in the manner in which it has now been brought forward—viz., for the purpose of eliciting information as to the best means of preventing its invasions in future. Whether, in fact, a different system of relief to the parturient female is to be provided from that which has been in operation in this city for more than a century, and which has been found to be attended with many and great advantages, certainly to suffering womanhood; and which, were it not indeed for the occasional occurrence of this formidable malady, would be superior to any other system which could be adopted.

Of such importance does Dr. Barnes, ex-President of the London Obstetrical Society, one whose name is familiar to all obstetricians, deem the discussion of this subject, that in the year 1865 he thus expressed himself, in making some remarks upon a paper read by Dr. Snow Beck on puerperal fever:—

"The president said that the subject was one of such paramount importance, that it deserved a special discussion every year. He believed that if attention were thus brought continually to it, that much would be done in the way of preventing this, the most destructive of all causes to puerperal women."—*Obs. Transactions*, Vol. vii. 1866.

In the year 1858 the Imperial Academy of Medicine of France deemed the investigation of this malady, which committed such fearful ravages in its hospitals, of such great import that it devoted several *séances* to its discussion; and when the names of Velpeau, Depaul, Beau, Piorry, Cruveilhier, Bouillaud, Trousseau, Cazeaux, Dubois, Danyau, &c., &c., are mentioned as having taken part in this discussion, and contributed their experiences, it may be inferred that the subject was handled by no pretenders to science, but by men of world-wide reputation as pathologists and obstetricians. The result of their discussions is contained in a volume published in Paris by Bailliere, 1858. I shall have occasion to allude to it again in making some quotations from it which bear upon the present subject.

We must not suppose, however, that the investigation of this disease has been neglected by the earlier or later writers, for we have been inundated with works upon the subject from the time of Strothers, who was the first to term the disease puerperal fever, to the present day. Indeed I do not exaggerate when I say the name of works upon this disease is legion, and no greater proof can be given of the great attention which this subject has received at the hands of medical men than the multiplicity of essays and volumes upon it. It has in fact received more attention, I think, from medical writers than any other affection.

Dr. E. Kennedy's paper may be said to consist of four parts or divisions, viz.:—In the first part or introduction he discusses the probability of all zymotic poisons being ascribable or referable to one common

principle, modified by a variation in natural or physical circumstances or conditions.

In the second part he treats of the disease, or rather the various forms of the disease, and details many cases illustrative of the different forms of metria, such as had occurred in his experience, and his treatment of them.

In the third part he lays down his thirteen propositions, from which he makes his deductions; and in the fourth he proposes his remedies.

Now with regard to the first supposition, for in the present state of our knowledge of this subject we cannot call it by any other name, although there are many facts and circumstances which may lead us to the belief that those zymotic diseases may have one common origin, and that circumstances may modify the one poison in such a manner as to cause so many different developments of it, and although it is a matter of notoriety that at a time when metria is epidemic other zymotic diseases are very often prevalent, yet we have no marked data sufficient to warrant us in coming to the conclusion that there is any one single zymotic influence, and that all those diseases spring from or are developed from the same germ. Hence it is better in the present state of our knowledge of this obscure and difficult subject not to be positive, nor draw deductions which are determinate and which are not warranted by facts. Indeed my friend Dr. Kennedy, I must say, is not positive upon this matter, but is content with saying that should the principle of *isomerism be adopted* the difficulties in accepting the idea of a common morbific poison would be lessened.

The second part of his paper, or, to speak in more correct language, the second portion or division of the first part is devoted to practical suggestions, and the diagnosis of metria or puerperal fever from its congeners.

Dr. E. Kennedy recommends the use of the lancet in ordinary metritis or metroperitonitis, and states that those are the cases which bear depletion best, and that the use of the lancet is too much neglected in them. He says that twelve to eighteen ounces of blood extracted from the arm, followed by eighteen to twenty-four leeches, will often relieve and subdue all inflammatory symptoms, and the pulse that was hard, contracted, and compressed will, as in ordinary peritonitis, rise to a more rounded resistance. He says in the puerperal fever of the present day the use of the lancet is rarely admissible, local depletion must be our sheet anchor, but in having recourse to it the secret of success is to reduce the pain by repetition of leeching before reaction has had time to establish itself, &c., &c.

With regard to venesection I differ *toto cælo* with Dr. Kennedy, and in no instance would I attempt or allow the use of the lancet. As well might you bleed in typhus on account of the headache as in this disease, on account of the abdominal pain, which is but the local manifestation of

general poisoning of the whole system. Venesection has been recommended by men of great reputation—Denman, Hay, Clarke, Dewes, and many others; but, alas! I have not the least doubt in my mind that many were killed by this treatment, and that perhaps the great mortality which has attended this disease may be accounted for by the antiphlogistic plans of treatment recommended by our predecessors (who loved the lancet not wisely, but too well), and who, had they been content with trusting more to nature and less to physic, might have had more success in their practice. The same may be said of leeching, which must be practised with extreme caution. No doubt in cases of pure metritis, or peritonitis, or metroperitonitis, leeches cautiously applied over the affected regions are useful; but where puerperal fever or metria prevails, I would not follow Dr. Kennedy's advice to leech and leech again *coup sur coup* until pain is abated or altogether removed. I would much prefer to trust in opium, poultices, removal of the patient to a purer air, and watching the moment to exhibit brandy, wine, quinine, thus endeavour to prevent the tendency to death which is by exhaustion. Those who have witnessed real puerperal cases will call to mind the rapidity with which tympany, diarrhea, effusion into the bronchial tubes, collapse set in, and no person now-a-days would have the temerity to order from sixty to one hundred leeches to be applied to the abdomen of an unfortunate woman whose whole system is poisoned by this metrial poison, whatever it may be. Yet such was the practice recommended by Campbell, who, not content with one hundred leeches, afterwards encouraged the bleeding by stupes, poultices, &c. Dr. Collins recommends the use of three or four dozen leeches, followed by the warm bath. Can we wonder for a moment that such a fearful mortality attended the disease? Is it not in fact a matter of surprise that any poor victim ever recovered from such treatment? I have read in works upon this subject cases in which I have not the slightest doubt in my mind that the patients were—I will not say killed,—but, to use the most charitable phraseology, they were hurried to the grave.

The same may be said of calomel, and other preparations of mercury. In simple peritonitis or metritis it may be admissible, but we must be very guarded in its exhibition, even in those cases. Nature never sets up an inflammation of this kind without some object. By our interfering with the efforts of nature we may do premature mischief in even mild cases. But what can I say of the treatment recommended by Dr. Kennedy at pages 274, 275 of his paper. He says, "When metria or peritonitis were to be anticipated, as when the disease was prevalent, after manual interference in labour, or for the removal of the placenta, my habit was to commence at once after the labour was completed with small and frequently repeated doses of mercury—a grain or grain and a half of grey powder every third or fourth hour, and even, in more

suspicious cases, applying mercurial ointment to the armpits and groins. By those means, without irritating the patient's system to any serious extent in the course of forty-eight or fifty-six hours, a slight mercurialization may be perceptible, often before the time at which the disease could grapple the patient." And then he goes on to say:—"Should metria show itself by pressing mercurials a little more freely the system may be speedily brought under its specific influence, and the disease be thus speedily or easily checked, if it had not been prevented. I have only," he says, "on one or two occasions seen puerperal fever prove fatal after mercurialization showed itself, and in them the action was complete. But, on the other hand, how often have I seen mercury fail in every dose in producing the slightest approach to its specific effects." He then mentions one case, where by accident it did produce its specific effects, and the woman, already mercurialized, was attacked by metria, and she died on the third day.

This line of practice, I must confess, appears to me to be most erroneous, and, if adopted generally, is calculated to lead to most injurious results. What would be thought of the surgeon on the battle field who would commence to plunge his knife into the most vital parts to probe for balls which had never penetrated, or who would amputate limbs for injuries never received? Would he not be deemed insane, and be quickly removed from his post? Yet exactly similar does it appear to me is the practice laid down for our guidance by Dr. Kennedy. He recommends you to treat the disease before it comes; to saturate your patient with mercury; to reduce, in my opinion to nil, the chance which the poor patient has of recovery, should she be unfortunately attacked.

It is a sad thing to reflect, sir, that in such an advanced age as this is, such doctrines should be taught, and that, too, by so distinguished a member of the profession as the ex-master of the Rotunda Lying-in Hospital, one whose experiences extended over a period of ten years as assistant and master, and under whose observation so many thousands of cases had passed. Nay, as Dr. Kennedy stated a few evenings since, he had been thinking on this disease for forty years; so that the opinions and doctrines laid down by him are not those of a young and raw enthusiast, but of a philosopher of forty years' standing.

I shall now, sir, come to the really important part of the paper, which was commenced on the second evening of the discussion, and I have only to regret that Dr. Kennedy should, in the latter part of his first evening's paper, have descended from his chemico-pathological Pegasus, and condescended to enter into the details which occupied a considerable portion of the evening.

The second part of the paper commences with the statement, that the whole family of puerperal diseases—puerperal fever, metritis, peritonitis, pleuritis, phlebitis, arthritis, pyemia, purpuric or cerebro-spinal metria,

traumatic metria, erysipelas, and hospital gangrene—may be all classified under the head of metria. To this statement, in my opinion, no objection can be possibly entertained, as although in different epidemics different pathological lesions had been observed, yet still, for the sake of simplicity, and for the reason that we know so little about it, or *faûte de mieux*, as the French say, we shall accept this proposition, and agree with Dr. Kennedy, that metria is to be assumed as representing all the different manifestations or developments of the same poison or zymosis.

On this broad foundation of zymotic metria he has raised up the thirteen now celebrated superstructures or propositions, which, being in print, must be familiar to the Society.

No. 1. There is no rejecting this proposition, in my opinion, and I believe it will meet with universal acceptation.

No. 2. This proposition is difficult to understand in many respects, but it means exactly this, if I interpret Dr. Kennedy correctly, viz., that any parturient female may generate a poison, then absorb or imbibe it herself, or those in her neighbourhood may be infected from this poison generated by her.

Now this theory of the origin of the puerperal poison and its elaboration, which Dr. Semmelivies was the first, I believe, to propound, and which Dr. Kennedy assumes as a matter of certainty, is in fact the keystone of the arch of this proposition, and is one the perfect truth of which should be established before it is given out *ex cathedra*, as it is in this proposition. Up to the present time, I think (and I speak under correction), this explanation only merits the name of a very ingenious theory to define the mysterious origin and mode of propagation of zymosis.

If this explanation of the origin of metria be the true one, it would differ in some respects from other zymotic poisons. Take scarlatina, for instance. Will anybody assert that it is produced in this manner? Does a person in a healthy condition become the generator firstly of scarlatina and then the imbiber of it? If so, this is a most remarkable illustration of *le cercle vicieux*. Now, I think that scarlatina, as an example, may be observed as making its appearance in two different ways:—Firstly, from some cause the poison is developed in the whole system, not in an organ, or, secondly, the person being exposed to the contagion is attacked, and the disease is thus developed. Are we, I say again, to suppose that the generator of a disease becomes the absorber of the same disease. The notion is a very unphilosophical one, and the fact should be well established before it is made a universal proposition.

Erysipelas may be taken as another, perhaps a more suitable, illustration of this poisoning, inasmuch as the position of the parturient female has been compared by Sir J. Simpson and others to that of a patient after undergoing amputation or some serious operation: an

analogy rather overstrained, in my opinion. However, *ad rem.* A patient is in hospital for some wound or accident; there is not a case of erysipelas in the ward; suddenly, from some cause, the patient is attacked by shiverings, erysipelas makes its appearance; and Dr. Kennedy would explain this phenomenon by saying that the patient generated a poison, and afterwards absorbed it. Now is not a more probable explanation of it this, that pyemia took place from the zymotic condition of the air of the hospital, and that the blood thus poisoned exhibited those manifestations. The laws of zymosis are so little understood, however, that up to the present we cannot, as I have said, acknowledge that we are certain of the precise means by which blood poisons are developed, and it would be like building a house upon a foundation of sand to found a theory of such importance upon a mere hypothesis.

No. 3. Not agreeing exactly in the view which Dr. Kennedy entertains as to the mode of generation of metria, and which he seeks to impress upon the Society, and also because I think that the very statistics upon which he relies are opposed to his ideas, I must withhold my consent to this proposition.

If we take the statistics of the Rotunda Hospital, and divide the years into groups or series, we find that the reverse of this proposition is true, for it is a fact that the mortality was less when a large number were admitted into the hospital than in similar groups of years when the number of patients was less. The very group of years upon which Dr. Kennedy relies so strongly, viz., the last fifteen years, shows the highest mortality, whereas those groups of years in which the greatest number of females were admitted shows the smallest death-rate. This part of the question has been so recently and so satisfactorily made evident by Dr. M'Clintock in his able and argumentative observations, that it would be a waste of time to repeat now what he has placed in such a clear light before us; but I may be permitted to say, that the statistics which I have copied from another source, viz., the mortality from puerperal fever occurring in six of the French hospitals, and to which I shall again refer, amply corroborate this important fact, viz., that the death-rate does not seem to bear a direct ratio to the numbers admitted.

No. 4. This proposition, in my opinion, involves a paradox, for it affirms that where a large number of patients are delivered under the same roof the disease finds its habitat, appearing and reappearing at uncertain intervals.

By the habitat of a disease we mean, in fact, the place of dwelling of it; and we must believe, if this hypothesis is true, that metria is constantly in the wards of a lying-in hospital. If it be, therefore, constantly present, or lurking in the atmosphere, how do we reconcile this with the well-known fact, that the disease only makes its appearance now and

then, appears and reappears at uncertain intervals, as Dr. Kennedy states.

If this supposition were true, every patient entering a lying-in hospital would be almost certain to be attacked. But is such the case? No. Intervals longer or shorter generally elapse between the occurrence of outbreaks of this disease, and sometimes it will remain in abeyance for a very long period. Dr. Kennedy has not framed this proposition in very logical language, and I think that it scarcely requires me to say that the proposition contains its own refutation, as a disease to have its habitat, must be always present, therefore it should never disappear.

No. 5. The language in which this proposition is couched is also vague and unphilosophical, for he says that its appearance, although apparently capricious, is not unfrequently traceable to the occurrence of other zymotic diseases, to a general unhealthy state of the hospital, &c., &c.

Now here, in my opinion, Dr. Kennedy is completely at variance with his previous statements. He asserts, in his first and second propositions, that metria is a parturient poison. This I believe myself to be true; but now he asserts that it is traceable to the occurrence of other zymotic disease. He does not specify what, but he in this proposition destroys or makes away with the individuality of metria. If such be not his opinion, his words to my mind convey that meaning, and I think that most of those who have carefully read his paper will agree with me in the interpretation which I put upon his language.

What, may I ask, is the meaning of the word *traceable?* It means to be traced to. Traceable, then, to what? Is it to typhus, to scarlatina, to variola, to erysipelas, or any other zymotic disease? Here Dr. Kennedy leaves us to our own imaginings. He throws no light upon this subject, so full of mystery, but is content with uttering a platitude, which, in my opinion, has neither meaning nor truth, and which does not in the least strengthen his position. He, in the first instance, endeavours to prove the individuality or essentiality of metria, in which, I think, he is correct; and he then, in this proposition, insinuates that it is traceable to, or has its origin in some other zymotic disease. This, I must say, appears to me to be a most glaring inconsistency, and one proposition negatives the other.

No. 6. This proposition has long been the battle ground of two great parties, and arguments have been adduced for and against the contagiousness of metria, which would be a work of supererogation to repeat. Many of those cases have, in the course of this discussion, been mentioned, upon which the contagionists most strongly rely, and certainly many of them are very remarkable, and are such as to induce us to have a strong faith in the doctrine of contagion. On the contrary strong arguments have been given by other writers, which, to a great extent shake the doctrine of contagion. I will not enter here into them;

they are amply discussed in almost every treatise upon puerperal fever, and are entitled to the most searching inquiries. As regards my own experience of this malady, I had opportunities, unfortunately—like all those who have made obstetrics their special study, and who have had large hospital experience—of seeing this scourge in all its forms and types, in all its degrees of severity, and I must candidly confess that although I do not disbelieve in the doctrine of contagion, yet that I think contagion is not the strongest element in continuing this disease in hospital. I have seen the disease attack perhaps one, or two, or perhaps more, out of several patients in the same ward, all of whom were healthy. Then it would make its appearance in another part of the house. To be sure, every precaution was taken, and the patients were removed to separate wards when the disease had assumed, as it always did, I may say, in the true metria, a bad form. But still I think that if this disease were so highly contagious as it is stated to be, that ample time to propagate itself had been, in many of those cases, allowed before removal. Now, let it be distinctly understood that for one moment I do not class myself amongst the non-contagionists, but that, on the contrary, I have insisted, and will always insist, on every precaution being adopted to stop this scourge so fatal to lying-in hospitals.

With regard to this highly important question of the contagiousness of this disease, and its infectiousness, all the French physicians agreed about the infectious nature of it, whilst there was some difference of opinion about its contagiousness.

MM. Danyau and Depaul are of opinion that the true puerperal fever is of a highly contagious nature, and is not only transmissible or communicable from one patient to another, but that it is communicable by another person as the medical attendant or nurse. M. Depaul goes further, and thinks that, under certain conditions, it may be communicated to non-pregnant women; and he gives the subjoined very remarkable cases, in proof of his statement.

He says that, in the year 1839, a very severe epidemic of puerperal fever raged at the maternity, whilst under his management. On one evening, one of the female pupils, Mademoiselle D., whilst in the act of washing the genital organs of a patient, suffering from a severe puerperal fever, suddenly experienced a most painful sensation, which she referred to the emanations which she had respired on raising the bed covering, and she declared that she felt very ill. On that very evening she was attacked by rigors. The belly became very painful, the pulse small and frequent. Soon after, vomiting and diarrhea set in. The vomited matters were green, and all the other symptoms of puerperal fever were well marked. She died on the third day. On making a *post mortem* examination, all the appearances of puerperal peritonitis were manifest. He adds that this young woman was not in any condition

similar to that of a parturient woman, she being a virgin, and not at a menstrual epoch.

He also adds that there was a tradition of similar cases which had occurred at the maternity on other occasions. And he likewise refers to two cases cited by M. Tarnier—two sage-femmes of the maternity—who, in 1856, during the prevalence of the epidemic, were similarly attacked, and exhibited all the phenomena of puerperal fever. One of them died and the other recovered. The epidemic at the time was of such severity that it was necessary to close the maternity. In both of those cases the women were at the menstrual period.

M. Trousseau, speaking on the subject, thus expresses himself, quoting an essay by M. Lorain, entitled "On Puerperal Fever in the Female, in the Fetus, and in the Newly-born Infant." I wish that he had added amongst the wounded of both sexes residing in the proximity of lying-in hospitals; and he goes on to demonstrate the evil effects of this poisonous influence extending from those establishments to the medical and surgical wards of neighbouring hospitals.

M. Dubois is rather disposed to question the contagiousness of it, or rather the facts adduced in evidence of contagiousness by English physicians.

M. Guerard rather inclines to the opinion of M. Dubois.

M. H. De Chegoin does not believe in direct contagion, because he cannot think that the quantity of miasm carried in this way by a physician to a patient is sufficient to produce this disease.

M. Cazeaux and M. Legroux believe in the contagious nature of the disease.

We thus perceive that amongst those celebrated names there is a great difference of opinion as to the contagiousness of this terrible malady.

No. 7. This proposition, if allowed in its fullest sense, is very subversive—that is to say, if you accept it as meaning that it is peculiar to certain places, or, as Dr. Kennedy says, confined to certain localities in its occurrence. If you allow this, the lying-in hospitals would never be free from it. But we know, and statistics prove it, that all lying-in hospitals are at intervals free from this plague, and that the general rule is, that it makes periodic invasions, sometimes mild, sometimes of such horrible severity, that hospitals are obliged to be shut, and the whole interior deodorized, fumigated, &c., &c., as has happened in the Rotunda, Coombe, Vienna, and French Hospitals. Now, if this disease were endemic, as I have said before, in the strict sense or acceptation of the term, metria would be the rule, its absence the exception, in lying-in hospitals. Such is not the case, but the reverse is the case. Wherefore, I think, we can scarcely give in our adhesion to this very dangerous and insidious proposition.

No. 8. I certainly think that I have observed certain wards to be

more affected by it than others. This circumstance, however, may be explained to some extent. Some wards are more healthy, better ventilated, have better nurses, and other circumstances may also modify the amount of disease in different wards of a lying-in hospital.

What, however, does Dr. Kennedy mean by the first part of this proposition, where he says that not only is it confined in its occurrence to a given hospital: does he imply by this that certain hospitals are never free from this scourge whilst others pass unscathed? Does he mean to select a particular hospital or hospitals? I think, sir, that the history of this disease proves that *any* hospital where parturient women are assembled may be attacked by it. No doubt some institutions have had more misfortunes in this respect than others; but I hold in my hand tables of the results of labours in six of the Parisian hospitals, under the care of the most eminent men, and in every one of those institutions metria made its appearance during the same year, corroborating in this manner a circumstance which has long been observed, that when the disease has made its appearance in a large city almost every lying-in hospital is simultaneously (I qualify the expression) attacked.

Can this proposition, worded in so decided a manner, and conveying the meaning that one institution is, *par excellence*, the seat of metria, then be accepted as true? I think not. No doubt there may be, and must be, a cause why puerperal fever most generally selects a place where there is a large assembly of parturient women; but it must also be remembered that the disease commits occasionally ravages external to an hospital, and in places remote from one.

No. 9. With this proposition I think we must agree, if not altogether at least partially, as although metria is not absolutely impossible of occurrence in patient's houses, yet we all must admit that experience proves that it is of much rarer occurrence in them than in the large lying-in hospitals.

It is on this proposition and the succeeding ones—10, 11, 12, 13—that the battle is to be fought between those who advocate large hospitals and those who advocate small hospitals, or the hospitals built on the system of isolation.

No. 10. If we agree with this proposition, we at once concede everything, for it contains the very essence or kernel of the argument. He says it is not a disease observed to occur in small lying-in hospitals, or cottages, where only one or two patients cohabit in their lying-in.

Now, sir, Dr. Kennedy's own paper contains the refutation of this statement, for at page 295 he gives the mortality of

New Ross as 1 in 185,
Waterford as 1 in 295,
Limerick as 1 in 367.

Certainly, in the Waterford Hospital the mortality from metria is small, *i.e.*, in the second group of cases. Dr. Elliot states that out of 2,656 women delivered since October, 1844, only two have died from metria, or 1 in 1,328. But previously he states that in the first hospital opened there was a mortality from puerperal fever of 1 in 251. Now, if Dr. Kennedy had qualified this proposition by stating that this disease did not occur so frequently or in such a severe manner, I think no gentleman here would disagree with him; but he has asserted absolutely that it does not occur in small lying-in hospitals or in cottages, and he has given the best refutation possible, to his own statement, *i.e.*, by quoting the statistics upon which he relies for victory.

I will now, sir, with your permission, read a letter from my friend Dr. Griffin, who presides over the Killarney Lying-in Hospital:—

"KILLARNEY, *May* 13, 1869.

"MY DEAR DR. BYRNE,—I only got your letter to-day, so I fear my statistics will hardly reach you in time. I have read Dr. Kennedy's paper in the Journal. I think our hospital tells favourably for institutions of the kind, as during the time it has existed no case of puerperal has occurred, notwithstanding the number of bad cases admitted far exceed the average of the same number indiscriminately chosen from the labours occurring at patient's homes.

"I have seen some dozen cases of puerperal fever during the same period amongst out patients in a district whose average of births in the year would be 120.

"I am sorry I have not more time to give more accurate information, and remain, very truly yours,

"L. T. GRIFFIN.

"ST. ANN'S LYING-IN INSTITUTION, KILLARNEY, OPENED JANUARY 1, 1866.

Number of patients confined up to present date, viz., May 13, 1869,	139
Number of maternal deaths,	3
Hemorrhage in placenta previa, patient came in exhausted,	1
Rupture of uterus—elderly woman—(three previous bad confinements),	1
Peritonitis after prolonged labour before admission,	1
	3

The last case lived fourteen days after delivery by craniotomy, and had no well marked symptoms of puerperal fever. Other patients were in hospital at the time, and took no disease from her, though attended by the same midwife and ward-maid.

"I have to observe, that of those who enter the hospital many have

had previous bad confinements, and that the poor women in this neighbourhood are not very willing to leave their own homes to be confined, except when urged by fears arising from past experiences. Of the 136 cases of recovery of the mothers, 5 were delivered with forceps, 3 by version, 3 craniotomy, several were complicated by hemorrhage, retained placenta, breech presentation, face presentation, &c. The hospital is well situated on an eminence, and consists of a basement, middle, and upper story; the latter comprises two wards, containing nine beds, 15 feet by 13; large windows at each end of each ward, perforated zinc panes; a small ward between, with labour couch; well ventilated corridor. We have seldom more than four patients in together; once there were nine.

"I think the circumstances of the house nearly realize Dr. Kennedy's suggestion of cottage hospitals."

We see by this candid statement, that out of 139 cases there was not a single instance of a death which could be attributed to metria. The total mortality from puerperal accidents and effects was 3, or 1 in 46⅓. Now this case strikingly illustrates the importance of distinguishing deaths from metria from deaths occurring from other causes in lying-in hospitals. Suppose, for instance, that the death-rate of the Killarney Lying-in Hospital appeared in a statistical return as 1 in 46, it would appear a very large death-rate, and we would feel very much inclined to look upon this hospital as one in which metria had its habitat; but what is really the case—

1 Died from hemorrhage,
1 „ rupture of uterus,
1 „ peritonitis (simple).

There has not a single case of metria occurred since its foundation. I am sure Dr. Kennedy will feel gratified by my adding this instance to his catalogue of small or cottage hospitals, and to their freedom from metria.

Those, too, who look upon metria as an independent disease, epidemic in its nature, and not depending absolutely upon overcrowding in an hospital, will feel their position strengthened by the reference which Dr. Griffin has made to the outbreak of metria in a district away from the hospital, and which was of such severity that 12 out of 120 deliveries, or 1 in 10, died in their own homes. There cannot be a stronger proof or illustration of the occasional outbreak of the disease than this circumstance, and of its independent origin. It would be very difficult to conceive that those cases originated from a contagious source, as there was not a single case of metria in the hospital; or, if the disease did spread through a contagious influence, the contagion must have been carried in another way, either by some of the attendants or persons in communication with the

parties first affected, for certainly the hospital which had not a single case could not have been the nucleus of this poisonous influence.

I shall now, sir, with your permission, give two more illustrations of freedom from metria in hospitals which have not been alluded to by any previous speaker. My friend Dr. Tate, resident medical officer of the North Union Workhouse, with the sanction of Dr. Minchin, who has charge of the maternity department, has addressed to me the following letter, by which it appears that from the 31st March, 1857, to May 11, 1869, that is, for twelve years, 1,067 cases have been delivered in the maternity department :—

"Dear Dr. Byrne,—I should have sent this, as you desired, last night, only I thought it desirable to go through the books carefully. I only got them late in the evening. Numbers of these women had twins, and some who are marked married have marks of interrogation after them where Dr. M. thought otherwise, still I put them down, giving them the benefit of the doubt.

"Yours very truly,

"David D. Tate."

Number of Women confined in the North Dublin Union Workhouse.

From 31st March,	1857, to 31st December,	1857,	83
„ 31st December,	1857, to 31st December,	1858,	78
„ „	1858, „ „	1859,	86
„ „	1859, „ „	1860,	72
„ „	1860, „ „	1861,	93
„ „	1861, „ „	1862,	128
„ „	1862, „ „	1863,	84
„ „	1863, „ „	1864,	97
„ „	1864, „ „	1865,	67
„ „	1865, „ „	1866,	108
„ „	1866, „ „	1867,	72
„ „	1867, „ „	1868,	74
„ „	1868, to 11th May,	1869,	25
	Total, ...		1,067

"Of these 570 were unmarried.

"One, unmarried, got peritonitis on third day by fright from some friend, and died; 2 more from peritoneal fever, 1 from convulsions, and 1 from suffocative catarrh was dying on admission.

"Management of cases under the supervision of Dr. Minchin.

"For about seven years there was a large ward and adjoining closet set apart for lying-in women; now there are two wards higher up.

"No epidemic occurred during twelve years."

From this table it will be seen that the mortality

From metria was only	1 in 355⅔
From other causes,	1 in 533⅓
Total,	1 in 213⅔

Now, sir, if we compare this with the death-rate which is marked in the tables (page 293) of the Rotunda Hospital, there is a striking difference; and, by a curious coincidence, those twelve years are included amongst the fifteen years which have been selected by Dr. Kennedy to illustrate his point. In what way, sir, are we to account for this striking dissimilarity in results? I believe that an explanation may be offered which will be a solution. The patients who came into the Rotunda to be confined were chiefly citizens, many of them primiparæ, many of them unmarried. However, the workhouse maternity carries off the prize in this respect, for I find that out of 1,067, 570 were unmarried, or more than one-half. And Dr. Tate assures me that this estimate is even under the mark, as the benefit of the doubt was given in a great many cases. But the women who were confined in the workhouse were chiefly those in the prime of health, coming from the outlying districts. Upon this point I speak with reserve, as I have not accurate information upon it, but such was my experience of that institution for a short time that I had charge of it; whereas, those coming to the Rotunda many were already in a delicate state, enfeebled from disease and poverty, and in many cases were already tainted or infected with disease, as Dr. Denham has already pointed out in his remarks at a previous seance, or, at all events, they were predisposed to it.

The next maternity which I shall bring under your notice is that of the South Union Workhouse, which is under the charge of Dr. Jennings. On account of an accident I could not find out the number of births for the same period as for the other workhouse, but during the three years, viz., January 1, 1866, to March, 1869, the number of deliveries amounted to 396. The number of those unmarried bore also a very large ratio to the whole. The total deaths amounted to 5, or 1 in 79⅕. I could not ascertain exactly how many of those were from metria. My friend Dr. Burne took the trouble of examining the books, and the only one whose death could be positively attributed to metria was one; the others were the victims of disease and debility. The nurse of the wards, a very intelligent woman, assured me that the only occasion upon which there was anything like a dash or outbreak of puerperal

was about twelve years since, when a patient suffering from fatal typhus was brought in to be confined, and several cases occurred very soon after. In this maternity there are two labour wards; one contains six beds, the other five. The rooms are separate; they are on the ground floor, very clean, and form part of the large building on the right hand side of the large yard. The ventilation appears excellent, and a very fine orchard is situate behind the building in which the maternity is placed. There is one particular bed used as a couch, the patients are then transferred to the beds in the other rooms. The rooms are occasionally overcrowded, occasionally empty.

I now come to deal with the first annual report of the maternity attached to Sir P. Dun's Hospital for the year ending March, 1869. This maternity gives attendance at their own houses to females within a circumscribed radius. It is under the medical surveillance of my friend Professor Sinclair, and I need scarcely say that no more accomplished nor able obstetrician could have been appointed to the office.

The number of cases which have been attended externally is considerable, taking into account the short time in which the system has been in operation, viz., 336 from April, 1868, to March 31, 1869; of these only two women died, as the report says, not from parturient causes, or thus—

From metria, 0 in 336,
Other causes, 1 in 168.

This report certainly is only for a very limited period, less than a year, and, of course, it can be scarcely taken as an index of the constant death-rate from out-door attendance. Although the cause of death in the two cases is not mentioned, yet we can safely trust the correctness of the report. It bears evident testimony to the handiwork of that

. . . . "Light divine,
That deigns on dark '*obstetricy*' to shine."

I allude, of course, to the Rev. Dr. Haughton, who takes so great an interest in medical matters, and whose talents and exertions have done so much to stimulate and promote the science of medicine. As an instance of the beneficial results of out-door attendance it is very satisfactory, particularly when we remember that the gentleman who has the medical charge of the maternity is not one likely to conceal any defects, nor under-estimate any of the disadvantages attendant upon this system, he being the author of that remarkable and trenchant paper or essay, entitled, "Extern Maternities and Lying-in Hospitals, &c. &c. Reply to Article published in the *Dublin Quarterly Journal of Medical Science*, &c."

I shall now, with the permission of the Society, make a few observations.

Before doing so, however, may I be permitted to direct the attention of the gentlemen present to some points, which illustrate the peculiar mode of reasoning adopted by Dr. Kennedy, and which, to the best of my recollection, has not been noticed by previous speakers.

At page 305, he says:—"Let us further bear in mind that the increased mortality bears nearly a direct proportion to the increased numbers inhabiting each building."

Now, sir, I do not think that it requires any person to be a very abstruse or accomplished mathematician to perceive the truth of this statement. If, for instance, out of 100 patients 2 die annually, we should expect that out of 200 patients 4 will die annually, and so on in like proportion. Is it possible, I may ask, sir, to expect a different state of things?

Can we hope that the death-rate will *diminish* in proportion to the *increase* of numbers, thus, for instance, supposing that there be 1,000 patients, the mortality, according to this rate, should be 20. Pray, would Dr. Kennedy have us expect that only 5, or even less, should die?

Having enunciated this self-evident proposition, that the increased mortality bears nearly a direct proportion to the increased numbers inhabiting each building, Dr. Kennedy proceeds to draw, as it appears to me, the most extraordinary inferences or deductions, or, as he terms them, comparative results, for which I do not think we can find sufficient data in his paper.

He assumes the mean of the mortality of the three hospitals—New Ross, Waterford, and Limerick—as the standard by which to judge the mortality of all lying-in hospitals, this being 1 in 282. Now to this, I think, a strong objection may, *in limine*, be urged, which is this:—There is a considerable variation in the mortality of the three institutions, for whereas the mortality of New Ross is 1 in 185, the mortality in Waterford is 1 in 295, and that of Limerick is only 1 in 367. This for three small hospitals, I think, will be acknowledged to be a considerable variation. But, why has Dr. Kennedy not contented himself with selecting some *one* of these hospitals as the standard, instead of giving us the mean rate of mortality, deduced from the comparison of all three together? Why not, for instance, select the Limerick Hospital, whose mortality is 1 in 367, and say this is my standard, and any hospital whose death-rate is in a greater proportion than this is one which should be closed.

Or again, why should not the Waterford Lying-in Hospital, whose mortality is 1 in 295, be selected as the standard, and every lying-in hospital be expected to show exactly similar results?

Or why not select the New Ross Hospital, whose mortality is 1 in 185, and say, This is my standard; every lying-in hospital must have a similar death-rate.

But, on the other hand, let us suppose that a maternity can be shown whose death-rate differs from those exhibited by Dr. Kennedy, why should not this death-rate be selected as the standard. Let us take, for instance, the Killarney Lying-in Hospital. According to Dr. Griffin's account the mortality in this maternity is 1 in 46·3, although no death occurred in it from puerperal fever.

Dr. Kennedy, however, does not select any single hospital, but having collected a certain number, viz., *three*—and those the three mentioned in his paper—he strikes the average or mean of death-rates in all three hospitals, and he gives that mean as the standard by which to regulate the death-rate of all the hospitals in Europe.

Now, in order to show this point in a clearer light, and that this standard is not to be assumed as the proper one, let us take the mean mortality of the three lying-in hospitals mentioned by Dr. Kennedy, which is 1 in 282; and adding to this the mortality of the Killarney Hospital, which is 1 in 46, see what is the result? why that the standard death-rate is raised from 1 in 282, or ·354 per cent., to 1 in 223, or ·448 per cent., being an increase of ·26 per cent., or over ¼. Now, why should not this be taken as the standard instead of 1 in 282?

On the assumption, then, that the death-rate in the three hospitals adduced by him should be regarded as the proper standard, Dr. Kennedy proceeds to draw, as I have said, the most extraordinary inferences, or, as he terms them, comparative results, of which I shall quote only three examples.

No. 1. He says that in the Liverpool Hospital 2 out of 3 die who should not die.

No. 5. In the Dublin, 8 out of 9 die who should not die, &c.

No. 9. In the Paris General Hospital 17 out of 18 die who should not die.

Now, by what process of deduction does Dr. Kennedy infer that those 2 should not die in the Liverpool Hospital, that those 8 should not die in the Dublin Lying-in Hospital, or those 17 in the Paris General Hospitals?

I must confess, that after the most careful and repeated perusals of his paper I cannot find any premises from which those conclusions, so vital, so revolutionary, can be elaborated, except, indeed, the arbitrary standard suggested by the combined results of the three lying-in hospitals specially selected by him to the omission of others whose death-rate would have quickly raised this low standard of mortality to a level nearer to that of similar institutions.

It must not be forgotten that this is the cardinal point of the whole discussion, and that its certainty is assumed by Dr. Kennedy in order to establish his position.

What is this method of reasoning but the taking for granted the

conclusion, which at the commencement of the discussion he undertook to demonstrate. This proceeding has been always looked upon as one of the most inexcusable of sophisms, and has been commemorated by a name well known to logicians as the *petitio principii.*

Dr. Kennedy, moreover, in his indictment against hospitals, does not specify the cause of the mortality, and even if we were disposed to allow that his generalizations are correct, I think that we are warranted in concluding that the number of those who die from non-puerperal diseases, and from the accidents of labour, bears a very considerable proportion to the whole, and in giving statistics upon such an important question, this distinction should not have been admitted.

My friend, Dr. M'Clintock, in his elaborate and critical paper, has shown most forcibly the importance of this distinction, and in the tables which he exhibited, he has demonstrated in the most distinct and unequivocal manner, that the deaths occurring in lying-in hospitals from puerperal fever, and from other causes, are in fact very nearly equal.

He also has shown that in private practice—and upon this point the statistics are most reliable—that out of 95 deaths which occurred in the practice of Clarke, Cross, M'Clintock, Browne, Churchill, and Sir P. Dun's Maternity, there were :—

Accidents of labour, - -	37
Puerperal diseases, - -	31
Non-puerperal diseases, - -	27

Now, sir, this is a most important point, and in estimating the death-rate of any system cannot be overlooked.

I have alluded before to some statistics which have been published in the printed report of the discussion of the French Academy of Medicine. Although in Dr. Kennedy's paper, at page 295, he gives the general death-rate as 1 in 18½, yet he has omitted to give the particular death-rate as it occurred in each hospital during a succession of years.

I have copied the tables published in M. Depaul's communication. They embrace the returns for 5 years, and from 6 hospitals—the years are 1852, 1853, 1854, 1855, 1856.

In M. Depaul's communication before the faculty at Paris, he gives the mortality of the Maternité for 5 years, ending 1856, as 1 in 69.

In the Clinique d'Accouchement, for the same years, he

gives the proportion as - - -	1 in 37
Hôtel Dieu, - - - - -	1 in 38
Hôpital Saint Antoine, - - -	1 in 40
Hôpital Saint Louis, - - - -	1 in 116
Hôpital Lariboisière, - - - -	1 in 24
Total, - -	1 in 48

Relative Proportion of Accouchements and Deaths from Puerperal Fever.

I.—MATERNITÉ OF PARIS.

	1852		1853		1854		1855		1856	
	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths
January, .	296	–	230	–	260	–	334	–	291	1
February,	289	4	299	8	302	–	265	1	271	1
March, .	311	5	339	19	236	–	235	–	351	3
April, .	263	–	314	18	303	–	257	–	317	20
May, .	239	1	225	7	305	–	259	1	51	30
June, .	205	3	165	1	239	–	206	–	55	–
July, .	236	1	193	1	262	–	241	4	184	5
August, .	196	1	208	7	239	1	255	5	205	2
September,	211	9	177	1	236	–	31	1	213	13
October, .	200	19	226	7	252	–	84	–	119	13
November,	217	3	216	–	289	–	102	–	185	7
December,	197	–	257	3	262	–	195	–	272	4
Total, .	2,860	46	2,849	72	3,185	1	2,464	12	2,478	99
Mean proportn.	1 in 62		1 in 39		1 in 3,185		1 in 205		1 in 25	

Total Accouchments, - - - 13,836
Deaths known, - - - - 230
Proportion, - - - - 1 in 60 about

Relative Proportion of Accouchements and Deaths from Puerperal Fever.

II.—CLINIQUE D'ACCOUCHEMENTS.

	1852		1853		1854		1855		1856	
	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths
January, .	98	–	123	7	104	2	116	–	108	5
February, .	103	–	52	5	95	1	89	1	81	2
March, .	117	2	2	–	97	3	116	4	79	1
April, .	92	1	—	–	93	1	104	4	107	–
May, .	117	3	76	1	81	–	99	2	66	7
June, .	76	–	96	2	54	6	88	2	9	5
July, .	96	1	74	2	76	–	91	1	—	–
August, .	104	1	93	4	58	1	85	1	65	–
September,	108	5	91	3	70	–	138	1	36	12
October, .	105	1	87	–	90	2	123	1	—	–
November,	108	–	87	8	89	4	116	3	38	–
December,	109	8	69	–	96	2	101	6	411	–
Total, .	1,233	22	847	32	1,003	22	1,266	26	630	32
Mean proportn.	1 in 56		1 in 26		1 in 45		1 in 48		1 in 19	

Total Confinements, - - - 4,979
Deaths known, - - - - 134
Proportion, - - - - 1 in 37

Relative Proportion of Confinements and Deaths from Puerperal Fever.

III.—HÔTEL DIEU.

	1852		1853		1854		1855		1856	
	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths
January, .	82	1	88	1	122	4	109	1	110	4
February,	93	4	93	9	145	5	112	2	107	6
March, .	80	1	95	7	145	3	105	–	113	6
April, .	92	1	104	9	159	3	96	3	109	4
May, .	98	2	108	4	131	1	83	2	154	10
June, .	59	4	87	1	107	1	86	–	188	11
July, .	84	–	101	–	109	1	94	–	136	5
August, .	73	1	104	–	81	–	85	1	105	–
September,	77	1	101	–	112	3	122	2	107	–
October, .	90	–	102	6	108	1	97	2	235	15
November,	85	1	120	1	108	2	109	2	156	10
December,	87	–	105	1	112	2	120	1	121	2
Total, .	1,000	16	1,208	39	1,439	26	1,218	16	1,641	73
Mean proportn.	1 in 68		1 in 30		1 in 55		1 in 76		1 in 22	

Total number confined, - - - 6,506
Deaths known, - - - - 170
Proportion, - - - - 1 in 38

Relative Proportion of Accouchements and Deaths from Puerperal Fever.

IV.—HÔPITAL SAINT ANTOINE.

	1852		1853		1854		1855		1856	
	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths
January, .	4	–	9	2	17	–	29	–	31	1
February, .	8	–	6	–	12	–	21	–	35	–
March, .	4	–	12	1	27	–	30	–	33	–
April, .	8	1	5	–	27	–	31	1	27	–
May, .	6	–	7	1	20	2	28	–	32	1
June, .	7	–	14	–	23	–	26	–	31	–
July, .	7	–	14	–	33	3	27	6	28	1
August, .	7	–	7	–	25	–	31	–	39	–
September,	11	1	12	–	24	1	30	1	25	–
October, .	8	1	8	–	27	–	31	–	39	–
November,	6	–	14	–	26	2	33	–	28	2
December,	10	1	7	1	27	–	27	–	35	–
Total, .	86	4	115	5	288	8	344	8	383	5
Mean proportn.	1 in 21		1 in 23		1 in 36		1 in 43		1 in 76	

Total Accouchements, - - - 1,216
Deaths known, - - - - 30
Proportion, - - - - 1 in 40

Relative Proportion of Accouchements and Deaths from Puerperal Fever.

V.—HÔPITAL SAINT LOUIS.

	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths
January, .	52	–	60	–	78	–	65	1	64	–
February, .	46	–	51	–	69	–	67	2	64	–
March, .	57	1	60	1	73	–	71	–	83	–
April, .	48	–	71	–	71	–	68	–	75	–
May, .	47	–	55	1	71	–	62	–	73	–
June, .	68	–	38	–	65	–	63	–	63	–
July, .	51	–	59	–	69	–	55	–	65	–
August, .	55	–	63	–	64	–	58	–	73	–
September,	61	–	62	–	58	–	72	1	61	–
October, .	61	–	58	–	63	–	55	1	71	–
November,	51	–	57	–	63	–	62	–	58	–
December,	63	–	58	–	76	–	65	–	63	1
Total, .	660	1	692	2	820	–	763	5	813	1
Mean proportn.	1 in 660		1 in 346		0 in 820		1 in 152		1 in 813	

Total Confinements, - - - 3,748
Deaths, - - - - - 9
Proportion, - - - - 1 in 416

Relative Proportion of Accouchements and Deaths from Puerperal Fever.

VI.—HÔPITAL LARIBOISIÈRE.

	1854		1855		1856	
	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths
January, .	–	–	47	2	59	1
February,	–	–	37	3	64	4
March, .	6	–	32	4	65	7
April, .	11	2	36	–	60	1
May, .	9	–	37	4	53	2
June, .	24	1	21	1	61	2
July, .	16	–	51	2	57	1
August, .	19	–	39	2	37	1
September,	20	2	53	3	51	–
October, .	25	2	48	1	55	4
November,	30	–	55	–	62	3
December,	31	1	49	–	61	0
Total,	192	8	505	22	685	26
Mean proportn.	1 in 24		1 in 22		1 in 26	

Total Accouchements, - - - 1,382
Deaths known, - - - - 56
Proportion, - - - - 1 in 24

Now, sir, on inspecting these tables, it will be observed that the same apparent inconsistencies seem to have prevailed in those institutions as in those with which we are more familiar. Thus, during some months or seasons, there was a very small death-rate; during others a very considerable one. Again, the death-rate did not bear a constant ratio to the deliveries.

Thus, as an example, during those 5 years there were confined in the Maternitè, - 13,836 women.
The deaths were - - - 230 = 1 in 60.
In the 3 years there were confined in the Lariboisière, - - - 1,382 women.
The deaths were - - - 56 = 1 in 24.
Again, in the Hôpital Saint Louis, during the 5 years, there were confined, - - 3,748 women.
The deaths were - - - 9 = 1 in 416.

So much was M. Depaul impressed with this last fact, *i.e.*, the small comparative mortality in the last mentioned hospital, that he writes, "If it is difficult to understand why the mortality from puerperal fever is very trifling at the Hôpital Saint Louis, it is equally, or more so, when we take into account the different results that we observe at the Lariboisière; every body knows the excellent situation of this magnificent hospital, its chambers are large and well ventilated, the number of confinements is small, and still the proportion of cases of deaths from puerperal is very considerable."

The death-rate from all these 6 hospitals is 1 in 48.

In the year 1854, there were 3,185 women confined in the maternitès of Paris—there was only 1 death.

In the year 1856, there were 2,478 women confined—99 deaths.

Contrast the 2 years' death-rate in the same hospital:—1854, 1 in 3,185, or ·0314 per cent.; 1856, 1 in 25, or 4 per cent.

How are we to account for this? yet, it is similar to what has been observed in other institutions.

Take the Clinique d'Accouchement's as another illustration:—
In the year 1852, 1,233 women were confined—22 died.
In the year, 1856, 630 women were confined—32 women died; or,
In 1852, 1 in 56, or 1·784 per cent.
In 1856, 1 in 19, or 5 per cent. died.

Again, in the Hôpital Saint Antoine, the entire number of confinements was, for 5 years, only 1,216, which gives an average of 243 per year.

In this hospital, in the year 1852, 86 confinements took place—there were 4 deaths.

In the year 1856, 383 confinements took place—there were 5 deaths; that is, 1852, 1 in 24; 1856, 1 in 76 died.

Thus we perceive that the number confined in 1856 was far in excess of the number confined in 1852, but the proportionate death-rate was inverse.

The tables of the Hôpital Saint Louis are well deserving of observation:—

	1852	1853	1854	1855	1856
Total Confinements, .	660	629	820	763	813
Deaths, . .	1	2	0	5	1

Not a single death occurred in 1854, although the large number of 820 were confined in the hospital.

I think that those instances will be quite sufficient to quote in illustration of the fact of overcrowding not being generally contemporaneous with a high death-rate, and is exactly what we observe in our own institutions at home.

So much, sir, for the statistics derived from an inspection of the tables which I have copied from this printed report. I think that the information which they afford may be looked upon as most reliable, and there is moreover this peculiarity about them, viz., that an inspection of them will enable us to view, almost at a glance, the obstetrical work going for a period of 5 years, in the large city of Paris, and to examine the results of practice not only in the hospitals especially devoted to obstetricy, but also in those in which parturient women are assembled in institutions devoted as well to surgery and medicine; and we cannot fail to be struck with the fact, that in the large general hospitals, such as the Hôtel Dieu, the death-rate was not higher than in the special hospital, the Clinique d'Accouchements, it being in the former 1 in 37, in the latter 1 in 38, whilst in La Maternitè it rose above that of the Rotunda Hospital, during the same period of years. The mortality in the Rotunda for those 5 years was 1 in 88; in the Maternitè of Paris 1 in 60.

Having said so much upon the comparative mortality of those hospitals and our own, I will now sir, with your permission, and that of the Society, allude, in as concise a manner as is possible, to the opinions held by some of those experienced physicians as to the best means of arresting this disease, and limiting puerperal mortality. The question of treatment should not here be discussed, for I need scarcely say to you, sir, and those gentlemen assembled here, what I am sure is the experience of every one who has seen true puerperal fever or metria, that the question resolves itself into one of prevention, not treatment. It is so fatal that medicine has little or no control over it, and our treatment of it may be simply said to be this: to endeavour to keep the patient alive by wine, brandy,

ammonia, opium, iron, quinine as recommended by Beau, a medicine, however, which has not realized all that was expected and predicated of it, and of thus trying to prevent the tendency to death. In this respect, sir, however, I cannot conceive that it differs much from other blood diseases. Who has ever cured a case of typhus? Who has ever cured a case of small-pox? or, what medicines have the least effect upon real Asiatic cholera? I do not believe that we can, by medical treatment, cut short any of those diseases; they must go through their stages, and our highest art must be acknowledged to be, to guide, and to watch, to treat local symptoms which may be relieved, and to act in such a manner that nature may get time and opportunity to throw off the poison, or materias morbi. This, sir, I think all here will confess to be the only treatment possible, and we all must agree with Dr. Stokes, who stated on the last evening that the real disease is in most instances fatal. Indeed Dr. Stokes stated that he never saw a single instance in which recovery took place in private, although his practice extended over a period of forty years. Such, however, is not my experience, for I have seen some very bad cases recover, and I have no doubt that if we cease to bleed, leech, and give mercury, the proportion of deaths to recoveries will be in a decreasing series.

The question then, sir, on preventing the disease being acknowledged to be the all important one, and that which has raised the present discussion, let us hear what our French confreres say upon this subject.

Monsieur Trousseau, comparing the conditions of those women who are confined in our maternities with those in which country women are placed, says that amongst the latter they are confined, but they do not die (*on accouche et l'on ne meurt pas*), and this in spite of overcrowding, filth, and the dirt of their apartments in which they are confined; but he quotes the following facts to show that the disease does not seem to depend on overcrowding, viz.: at the Maison de Sante d'Accouchements in the years 1837, 1838, 1847, 1848, the number confined was 14,217; deaths, 312; mortality, 1 in 45. In the years 1852, 1854, 1856, the numbers confined were 11,482, the deaths were 638, or 1 in 17. He believes that contagious miasmata, producers of surgical typhus and puerperal typhus, are constantly lurking in surgical and obstetrical wards, and that at certain periods, and without a cause which we can define, they germinate in the system, and suddenly explode in either a sporadic or epidemic form.

M. Depaul says that, from what we know about puerperal fever, that it is nearly exclusively developed in houses or hospitals, in which are collected in large numbers women in their confinement; and that the cases observed in civil practice are in general only the emanation of the epidemics first concentrated in certain hospitals, that once the poison is developed, it is transmitted more surely and more fatally according as the number of

women assembled is considerable. His advice is to disseminate women as much as possible in different establishments, and, better still, to attend them at their houses. He says :—"I entertain the most profound conviction that this is the only means of causing the disappearance of, or notably diminishing those murderous epidemics, which come on periodically to carry grief into families, and to sadden the physicians who, having nothing to oppose to this disease but their uncertain medicines, only intervene to show their want of power."

M. De Chegoin says also that a number of women assembled together prove a focus of contagion.

M. Cruveilhier says, in the most forcible language :—"I state, with all the conviction of truth, that there is only one course to take in order to prevent the periodical return of those murderous epidemics—that is, the suppression of the great lying-in hospitals; the replacing them by attendance at the women's homes; in addition to which may be added a number of small hospitals, outside of Paris, capable of admitting twelve, fifteen, or twenty lying-in women, in which each woman may have a separate apartment.

M. Danyau says that lying-in hospitals are not only institutions for relief, but, moreover, they are educational establishments; and that so far from suppressing them, he would increase their number, the only way of doing away with large collections of lying-in women, and of thus rendering the invasions of puerperal fever more rare and epidemics less severe; at the same time thus multiplying the foci of instruction. He pays a high compliment to the Rotunda, which he had visited, and which he terms, "Le magnifique hôpital d'accouchement de Dublin."

He says, however, that everything he had seen forced the conviction upon him that small hospitals are preferable to large, and that even those can only be kept in a sanitary state on the condition of never being always full, of filling the wards by turns, and by adopting the best means of ventilation, deodorization, cleanliness, &c., &c.; and when signs of the epidemic make their appearance, by instantly closing the wards; and he finally recommends small maternities to be built as far away from *ordinary* hospitals as possible.

It is not surprising that puerperal should be so frequent in the maternitè, for M. Danyau mentions a fact which is startling, viz., that in the year 1837 the pressure for admittance was so great that each salle or ward could be only evacuated twice during the year.

When this is compared with the system carried out in the Rotunda, I think the advantages will be found on the side of the latter, each of whose wards is thoroughly washed, cleaned, fumigated, ventilated, and whose bedding is renewed after each admission of patients.

M. Guérin also demands the suppression of maternities.

Now, what is the opinion of M. P. Dubois?

He says that the radical suppression of maternities could not be carried out in the actual condition of society; that there is no great centre of population which can do without them.

He also states that it is his belief that in the majority of cases the epidemic influence is in the first instance external, and that it only penetrates into the hospital after having manifested itself in the city; and he quotes the epidemic of 1836 as a striking example.

In this epidemic the first cases which he observed were outside the hospital, and the hospital was subsequently attacked. M. Dubois is of opinion that the suppression of maternities would make no difference in the actual state of matters, as regards the seat of the first epidemic manifestations, and would be a matter to be regretted, and would not be attended with equivalent results. He also quotes the instance of the fearful epidemic which attacked the village of Brackel, where, out of 28 women confined, 13 were attacked with puerperal and 12 died, and 2 only of the children of those women survived.

He also objects to their suppression, on the ground of their being educational establishments, and proposes to build, or annex to other hospitals without mixing them up together, buildings capable of receiving 600 or 800 women in their confinements annually.

To divide these buildings into two principal parts, equal in size and united by side galleries.

To divide each into chambers capable of holding ten beds, allowing an ample supply of air to each.

Now, in fact, he does nothing more than recommend a system which has been for a long time carried out in Dublin, except that less than eight beds are in each ward; he certainly refers to the arrangement of the Dublin Lying-in Hospital in very flattering terms.

M. Legroux says that when an epidemic shows itself in a maternity it nearly always makes its ravages elsewhere, and this even under the most favourable conditions.

He says:—Do we not see in an hospital on one day a whole series of women attacked, and those confined on the following day preserved? but the overcrowding exists as well for the one as for the other. Why, then, he asks, is the assembling of lying-in women more deadly on one occasion than another, when all the hygienic conditions are the same; and I think he hits off the answer when he says, it is that the *epidemic cause* is independent of overcrowding; but it strikes precisely there where its victims are in the greatest number.

We thus perceive that there is considerable difference of opinion amongst the Parisian physicians about the propriety of closing the lying-in hospitals, and that names of great celebrity are opposed to one another, as advocates of different systems.

What then are we to deduce from a consideration of all the facts

before us? I think that we may be fairly justified in concluding, from what has been elicited in the course of this discussion, that there is no doubt that there has been an increase of metria in the hospital during the last fifteen years, the period selected by Dr. Kennedy upon which to found his death-rate in this institution, and that the death-rate has been higher, during this period, than during any similar consecutive number of years.

That Dr. Kennedy has selected the most unhealthy years, upon which to strike his average death-rate for the hospital, but that he has omitted to strike an average death-rate from other most healthy groups of fifteen years.

That he has failed to prove that metria does not sometimes attack small lying-in hospitals, and even from his own tables it is evident that there is a considerable variation in the death-rates of small or cottage hospitals.

That circumstances would warrant us in believing, that except under conditions favourable to the developement of it, there is not a very intimate connexion between the overcrowding of hospitals, and the occurrence of epidemics of metria, for we have seen that at times when hospitals have been overcrowded, the death-rate has been low, and that, on the contrary, when on many occasions the hospitals have not been overcrowded, the death-rate has been very high.

That the very group of fifteen years selected by Dr. Kennedy proves this. During this period the admissions to the hospital were far in diminution of those of previous years, and the deaths were far in excess.

That the high death-rate in several of the French hospitals does not seem to have been dependent upon an overcrowded condition, nor to bear any relation to the size, position, or sanitary conditions of the several hospitals, for we find that in one of the best situate, most modern, and best ventilated hospitals, La Lariboisière, the mortality during an epidemic was higher than in the Hôpital Saint Louis, which is an old hospital, and is situate in a poor and crowded part of the city, and cannot boast of all the improvements, in a sanitary point of view, in which the other excels.

We find that puerperal fever prevailed epidemically in all the Parisian hospitals in which accouchements take place, and was not confined to one or two; but attacked them all. This circumstance is strongly corroborative of the epidemic nature of metria, for otherwise it would most probably have selected some particular hospital.

That in the maternity attached to the two Dublin workhouses, the death-rate in one, on an average of twelve years, was only 1 in 213. Whilst in the other the death-rate was 1 in 79, although the conditions were in both cases very nearly similar; it must be remembered, however, that in the latter the death-rate was founded upon the statistics of only three years.

That in the Killarney Maternity the death-rate was high, viz.:—1 in

46, although there were no deaths from metria. This very instance shows the importance of distinguishing deaths which occur from metria, from those resulting from the complications and accidents of labour.

There cannot, too, be a more convincing proof of the existence of this disease independently of hospitals than the statement of Dr. Griffin, that it was prevalent in the neighbourhood and carried off many, whilst the hospital was totally free from it. This circumstances is exactly in accordance with what has been observed elsewhere, as in the remarkable epidemic which attacked the village of Brackel, in Westphalia, and which committed fearful ravages. Another instance was mentioned on the second evening of the discussion by Dr. Denham, who read an account of a very remarkable epidemic, which about twenty years ago traversed the course of the river Barrow, and was very fatal to those attacked.

That there is every reason to believe that the disease is for the most part epidemic in its nature, but that just as cholera is known to make its appearance most frequently where large numbers are assembled, although we are in ignorance, why such should occur occasionally and not always, so the metrial or puerperal poison appears to have an affinity for that condition of the air which is engendered, when large numbers of puerperæ are assembled, and is most frequently manifested under such circumstances, yet such is not always the case, and it is frequently seen to make its appearance under an opposite condition, or where there is not a very large number assembled, as in many of the instances referred to.

That although the same reliance is not to be placed upon statistics from external sources as upon those derived from the well-kept records of intern lying-in hospitals, or rather that we have not proper data as yet for statistics from such external sources, yet there can be scarcely a doubt that there must be a larger death-rate in large lying-in hospitals than there is in private practice, and in out-door attendance upon patients at their own homes.

So far, therefore, as our present knowledge extends, we are unable to account for all the inconsistencies which appear in studying this disease. We see on the one hand the disease prevalent in hospitals which are not overcrowded, on the other hand we see it absent when the hospitals are so. We see it absent in unhealthy hospitals. We see it present in the most healthy. We see the most feeble persons escape. We see the strongest attacked, and succumb to it. We see it epidemic, and hospitals escape on the one hand. We see it make a rapid invasion into an hospital and then as suddenly cease. Whilst not a single case will be seen outside the hospital.

Such is my experience of this disease, and, from what I have read of it, such appears to be the experience of others. Whilst, therefore, agreeing with those who are of opinion that in an educational point of view large lying-in hospitals possess the greatest advantages, and are, moreover,

attended with the greatest benefit to the poor in many respects, yet I think that, on the whole, external attendance on an extensive scale, combined with an internal accommodation limited in extent, will be found to be the best, and one the most free from a very large death-rate. This is the system I think to which we are drifting with the tide, and the success of the experiment tried at Sir P. Dun's, is one of the strongest evidences in my mind of this tendency. I think that a fair way of putting the question would be this, viz. :—would any of the gentlemen, experienced in lying-in hospital management, if they had the power, open a large lying-in hospital with we will say 100 beds. I think—I may perhaps be in error—that with their present experience, they would not select to have so great a number of beds, and that they would commence, and be most likely to continue, with a smaller number.

I must, however, decline to accept Dr. Kennedy's solution or explanation of the modes or propagation of metria as final, and I do not agree with him in thinking that the isolation of patients in huts in the garden of the lying-in hospital, and the closing of the noble wards of this grand institution, will put a termination to this scourge of lying-in women ; on the contrary I think that, by attention to ventilation and other matters of the kind, and by watching carefully over all the minutiæ, which is so necessary and important a matter, but above all by diminishing the number of admissions, and giving a larger out-door attendance, and which I am sure the present master, my friend, Dr. G. Johnston, aided by his skilful assistants, will do, that, at all events, the death-rate will not increase, and I cannot anticipate that it will be a larger death-rate than it would be in huts if the plans recommended by Dr. Kennedy be carried out—plans founded upon hypotheses, and based upon data not sufficiently proved. With regard to the hut system recommended by Dr. Kennedy, I think the case mentioned on the last evening by Dr. Stokes, quite settles that question. For he adduced an instance of the fact of its being a constant resident at Ballarat, in Australia, amongst the settlers who lived in nothing but huts. So that I think we may say little more about this ; but I may take this opportunity of expressing the feeling of admiration—a feeling shared, I opine, by every gentleman who had the good fortune to be present on that occasion—of the lucid, philosophical, and logical observations which were made upon that evening in reference to this important subject by our illustrious visitor, Dr. William Stokes.

Dr. Charles F. Moore said :—

Mr. President and Gentlemen,—The subject, puerperal fever, has been so fully and so ably handled by physicians of such eminence during the debate under the consideration of the Society, that I shall not venture to occupy the time of this meeting with any remark except in relation to the habits of, and the conditions of the dwellings of the community, that

seem to me to predispose to puerperal fever, as well as to erysipelas, scarlatina, measles, and typhus.

My reasons for endeavouring to show what have appeared to me as the cause and effect, in the production of those diseases, commonly now called zymotic, is that for some years I had in my care, not only sick, but also numbers of strong and healthy persons, to preserve whom, in an efficient and healthy state, was my especial duty, and that under circumstances of climate, &c., highly trying to even the strongest.

Also, for a considerable time, more recently, I have been in contact with disease, so to speak, not only among the masses of our Dublin city population, but also among those of London, and in two rural districts.

I must express my dissent from the opinion of Dr. Evory Kennedy in reference to the injurious influence exerted upon the health of a community by the hospitals, whether lying-in hospitals or those for disease generally.

With your permission, Mr. President, I will quote a work written by a Fellow of the College of Physicians in Ireland fifty-nine years ago, upon the value of hospitals in the prevention of disease. The late Dr. Martin Tuomy, in "A Treatise on the Principal Diseases of Dublin," writes thus:—

"Typhus, or contagious, or spotted fever, is another disease which I have treated of pretty largely, in consequence of its very serious nature, and of the ravages which it has been known to commit in Dublin, until the institution of the Fever Hospital in Cork-street has almost banished it from the higher ranks, and greatly reduced its frequency and malignity amongst the poor. The blessings of that great monument of philanthropy are incalculable; and this is less to be wondered at when we consider that it is in a great degree directed by the characteristic industry, the rigid honesty, and the unlimited benevolence of the Quakers."

"CAUSES OF FEVER.

"Great inanition, large evacuations, raw, spoiled, indigestible food; thus fever is more usual and severe in seasons when there is a scarcity of corn and other food, and consequently when corn and other provisions are much damaged; foul, stagnant, corrupted air, as of dirty cellars and garrets; intense application of mind; despondency; want of sleep for a long time; sedentary life; great fatigue; cold and moisture; human effluvia; contagion. Two or more of these combined will cause fever with more certainty and danger than a smaller number. Thus, a gaol fever is a bad fever, and spreads with great virulence. To produce this fever are united confined foul air, an inactive sedentary life, and frequently great distress of mind; and such a fever, spreading its contagion, abroad, will cause fevers of a very bad kind. It might, perhaps, be also true that the quality of the disease being given, the sickness communicated

will be bad in proportion as the contagion is concentrated and malignant; and mild, as the contagion happens to be diluted."

The foregoing shows the value set upon hospitals in this city sixty years ago, by a physician in daily contact with disease, as a means for arresting the spread of disease of a contagious nature. I can add my own testimony and that of my colleagues as to the value of such institutions for the same purpose at the present time. This, too, has been my experience abroad, where pestilence has prevailed to an extent of which those who have only practised at home can have no practical idea. Thus in an African town of scarcely 70,000 inhabitants the deaths from cholera averaged 260 to 300 daily for many days.

In China dysentery in a severe form attacked the crew of the ship in which I at the time served, but on the removal of the cases to hospital, and the cleansing of the quarters occupied by the portion of the crew that had been attacked, the disease ceased.

We have been constantly instructed of late years that zymotic disease is immensely favoured by residence in hospitals, and that it is to such institutions the mortality in childbirth is almost wholly owing.

I will now for a few moments ask your attention to two or three points connected with this subject. Dr. Beatty has told us of the occurrence of metria in private practice, and so has Dr. Stokes and other most able physicians, and I doubt not the same has, to a greater or less extent, been the experience of every accoucheur and physician. Dr. Barnes, of London, stated lately, that 2,000 lying-in cases are annually attended under his supervision in the Royal Maternity Charity, and that it was not in that class of patients he found puerperal fever to prevail, as "for one case amongst these he saw ten amongst the easier classes," or, in other words, amongst those persons who never enter a lying-in hospital.

The Registrar-General of England and Wales has given a table of the mortality of women of the age of 25-35, which represents the period of life in which English women bear the greatest number of children, and it will be observed that the mortality of women at that age from all causes—please to observe—"is nearly *one* per cent (·992)."

Again, exclusive of certain districts, in which large lunatic asylums and hospitals disturb the results, there are fifty-eight districts in which the mortality of women of 25-35 from all causes exceeds 1·200 per cent.

Puerperal fever carries off a considerable number of the labour cases in the rural districts alluded to. The mortality of all women of the ages above named in several of those districts favoured by the absence of hospitals, actually exceeds annually 1·400 per cent.

Well would it be for those people whether in labour or not, if they could all enjoy the immunity from death stated to be enjoyed by the inmates of the small hospitals, New Ross, Waterford, and Limerick.

No unprejudiced reader, having heard Dr. Evory Kennedy's paper read, can peruse the extremely able and exhaustive treatise on puerperal fever published in the Cyclopedia of Practical Medicine, before Dr. Evory Kennedy undertook the responsible duties of master of the Dublin Lying-in Hospital, without regretting that Dr. Kennedy had not borne in mind all the evidence of an extremely contradictory nature there collected by Dr. Lee, not only from his own practice, but also from the writings and experience of eminent British and foreign authors on that important disease, the question of its causation, contagiousness, and its relation to hospitals, with a view to bring his great experience at the hospital, in the years of his mastership, to the solution of questions still undecided, now after the lapse of thirty-six years from the date of Dr. Robert Lee's paper.

I will now, Mr. President, ask your forbearance and that of the Society to solicit your consideration of what appears to me the true starting-point in our endeavour to lessen the chances of mortality to that most important class of patients—parturient women.

Every modern writer on the subject of the diseases incidental to that class of cases insists upon the strong predisposition to puerperal fever fostered by a relaxed and debilitated state of the system. The injunctions to keep up, by a well-adjusted and sufficiently tight binder, so long one of the essential doctrines of the Dublin School of Midwifery, form now one of the most generally taught precepts of a preventive nature at and after the actual period of delivery.

For like reasons every experienced practitioner has long sought to improve the tone of pregnant women, keeping in mind the important effect so exercised upon her welfare in her most trying time; and surely this Society does not require that I should tell it how injurious a system, one the reverse of tonic, is to such patients.

How much hemorrhage depends upon debilitated and relaxed fibre, and how much puerperal fever depends upon hemorrhage, and the many other severe accidents arising from exhaustion and debility.

I need not say how closely puerperal fever resembles, but in an exaggerated degree, the low state and many evils of parturition in the ill-fed, ill-housed, and mentally-depressed room-keepers of our large cities, even in times when no epidemic influence almost ensures their still more perfect receptivity.

Yesterday I saw a delicate primipara, whose dwelling and its surroundings appeared quite sufficient to have caused the state of extreme exhaustion and irritative fever in which I first found her, a few days after her confinement, with gangrenous sloughs over each trochanter, and with one greatly swollen knee-joint. A second mother, having an intensely painful mammary abscess, with prolapsus uteri and cardiac symptoms, lives in a house, also in the Liberty, that frequent cleansings

under the supervision of the city authorities have failed to render healthy, owing in a great measure, apparently, to the proximity of higher buildings excluding the air and light, and to imperfect sewers, coupled with over-exertion on the part of the patient, who is also nursing.

Another case was that of a young wife, three weeks a mother, whose high rate of pulse, debility, tendency to hemorrhage, and later tendency to bedsores, with miliary eruption and ulcerated nipples, caused me uneasiness for some days, more especially as her infant had aphthæ of the mouth and abrasion of the umbilicus. The train of symptoms in this case, though in many ways in pretty comfortable circumstances, I am disposed to attribute to a residence near the Liffey, and a naturally weak circulation.

Last November I was called to an artisan's wife who was lying in an ill-ventilated small back room, adjoining a yard where pigs were kept, in a court where many fatal cases of scarlatina occurred a few months before, and who had phlegmasia of both legs, with gradually increasing œdema of the body, face, &c., great prostration, and a low febrile state. I could not induce her to remove to hospital, or to a more roomy lodging. After some days she was removed to a room with a fireplace; but this step, and the best nourishment, stimulants, &c., her husband could procure, proved of no avail, as well as what medical treatment was pursued, and she gradually sank in three weeks from the first day I had seen her, leaving her infant a tolerably healthy child. She had been unable to suckle it from the day before I first visited her.

I shall not detail cases where three, four, and five of a household, and even of a family, have died lately of scarlatina to my knowledge, and in some of these cases the mother has been at the time pregnant. . . . Nor can I now trespass upon the Society with instances of fever spreading from pest-spots in our city, kept up by the scavengers' yards, and the depôts of much less extent of filth, nurtured with jealous care by dairymen, pig-owners, and others, the ill effects of all which are beginning to be appreciated by our working classes, as the letter which I shall now read, with your permission, bears witness to:—

"*May* 28.

"Sir,—I would wish to draw your attention to No. 5 Lower Clanbrassil-street where you have been visiting on last Saturday a woman and child that was ill you said that no one could get there health where the pigs are under there windows you where kind enough to say that you would report them but I think it has escaped your memory Sir I think if you would you would do a great act of kindness to the poor room holders there are ten famileys and 37 in number the house has never being without sickness the whole winter meassels scarlitian and at present there

is ghastick fever in it and no lobby windows that can be opened in it the man that owns says that he will keep them in spite of you or the board of health,

"I remain your
"humble servant."

Of the connexion between scarlatina and puerperal fever, few, I believe, entertain doubts. Should any such be present, I beg to refer them to the evidence given in the report of the medical officer of the Privy Council as to the outbreak of puerperal fever at Maidenhead,[a] where of 12 attacked 11 mothers perished. This outbreak points to three circumstances: First, the apparent connexion between the disorder and scarlatina; secondly, its apparent propagation by certified midwives; and lastly, the entire freedom of the district from anything in the shape of a lying-in hospital.[b]

I will not weary the Society by a detail of cases of exhausting uterine discharge, abscesses of the mamma, bedsores, irritative fever, &c., occurring in confined and otherwise unhealthy localities in Dublin. I detailed above some cases seen yesterday. To-day a case of chronic uterine hemorrhage in a poor artisan's wife thirteen years resident in a house near Longford-street, the hall and back-yard of which I found flooded with liquid sewage in a state of fetid fermentation—a state of things that shall, I trust, be promptly abated, as I have placed it under the notice of the sanitary police. The same house, too, is not supplied with water. Another case of exhaustion, with cardiac symptoms, in the mother, and gastric fever in a child, was seen by me to-day in Cuffe-street; another of gastric fever in a boy, and one of tonsillitis of an unhealthy character in a man I also visited in the same vicinity. In every case the surroundings of the sick people were such as would render a parturient female but too liable to puerperal disease.

Three years ago, when fever was prevalent in Dublin, I traced communication between upwards of one hundred and fifty cases of fever to one family of eight persons, who had fever close to the scavenging depôt to which I have above alluded. Many a time have I seen the spread of disease arrested by the early removal of the first case; and I am thankful to say this fact is becoming generally recognized by our labouring classes,

[a] In 1865. See also the valuable essays by Drs. Denham and Halahan.

[b] Professor Braun, of Vienna, has collected facts in connexion with puerperal fever, which tend to the belief that the disease occurs chiefly, if not entirely, from the epidemic constitution of the period.—*Medical Times and Gazette, March*, 1863.

Professor Stamm, of Berlin, has pointed out that the plague was annihilated at Cairo by throwing that city and suburbs open to the fresh air of the desert by levelling the hills that surround it.—*Ibid.*

In Greenock a very fatal form of fever hung about a part of that town sheltered from the breeze by an elevated hill.

who are beginning to feel the fatal results so often attending the still too popular custom of waking the dead. On the subject of cottage hospitals permit me to read the opinion of an eminent architect :—

"Dublin, 58, Lower Dominick-street,
"13*th May*, 1869.

"My Dear Sir,—Referring to our conversation respecting 'Cottage Hospitals' in the Rotunda Gardens, previously removing the present hospital and the Rotunda, &c., it appears to me a most doubtful and precipitous trial of a matter in which there has been as yet but a mere trifle of experience ; besides a plot of ground but 750 by 500 feet in the heart of a city would scarce be suitable for such, or afford a space for the thorough isolation of such buildings, or accommodating a number of patients approaching what heretofore is required. Besides, such buildings would be so out-topped by the surrounding dwellings that likely their ventilation, and even chimney draught, would be quite retarded ; and the removal of trees, and substituting diminutive buildings with yards and approaches, and all the work of receiving, attending, and cleaning, &c., &c., spread out to view, would surely be most obnoxious to the surrounding inhabitants, and even the public frequenting the locality.

"I may also add, that the very removal of a public building of some pretensions in appearance and position would surely be regretted by all meaning well for our city, together with

"Yours, dear sir, very truly,
"Sandham Symes.

"To Chas. F. Moore, Esq., M.D., Stephen's-green South."

If Dr. Kennedy's statement be capable of proof, when extended over a sufficiently wide basis of observation, "that the mortality from metria is annually increasing," which, however, I greatly doubt, this disease would then resemble cholera, diarrhea, dysentery, and diphtheria, all of which have been increasing in fatality for several years past, though surely this fact cannot be put down to the account of hospitals, but more probably to the pollution of rivers, and of drinking water generally from modern sewage arrangements.

In conclusion, allow me, Mr. President, to invite your co-operation and that of every member of the Society, including Dr. Evory Kennedy, to the great means, as I humbly submit it to be, of preventing puerperal fever—I mean the improvement of the sanitary tone of our masses, as well as of those in better circumstances.

Dr. F. T. Porter asked Dr. Beatty whether it was in the summer or winter that the cases of puerperal fever to which he referred had occurred?

Dr. Beatty said at all periods.

No other member having expressed a wish to address the Society, the President said:—

As no other member proposes to address you, it is now, I believe, my privilege as your President to close this debate with a few observations. It had been originally my intention to make a brief review not merely of Dr. Kennedy's paper, but of the entire discussion that it originated. I am sure, however, that you will all be much pleased at learning that I long since changed my mind on this point. To have done so in a satisfactory manner, considering the great length of the paper itself, and of the ensuing debate, would have taxed your time and wearied your patience to an inordinate extent, and would have been but a *crambe repetita*—a mere repetition of what had been already much better said. I subsequently thought of remaining altogether silent, mindful of the Eastern adage that "speech is silver but silence is gold;" but upon mature reflection, I came to the conclusion that upon so important a question, involving issues of such vast moment, I would not be justified, as a practical obstetrician, connected for nearly twenty-eight years with a midwifery hospital of no small importance, in withholding the expression of my individual opinion on some of the points raised in the paper under our consideration, to three of which, however, I shall at this late hour limit my observations.

Before I touch upon these special topics, I deem it right to refer to a matter which is in some degree personal to myself as President of this Society. It will be in the recollection of most of those present, that Dr. Kennedy on two occasions in the course of this discussion complained that it was conducted in a most irregular manner; that instead of it being carried on in accordance with the ordinary principles of debate—namely, by *ex tempore* addresses—previously prepared essays were produced and read; and further that no limit was placed upon the time to be occupied by each speaker. Dr. Kidd, our valued secretary, in reply to one of these complaints stated that these very points had been under the consideration of the Council of the Society at the outset of the discussion, and that they had unanimously decided upon the course which had been pursued, namely, to place no restriction whatever on either the mode of address or time occupied by the respective speakers. In that decision I fully concurred; and in fact had the Council not expressed any opinion upon the point, I should have felt it my duty as President to have taken upon myself the entire responsibility of permitting the deviation complained of—if deviation it be—from the ordinary principles of debate. Dr. Kennedy having reiterated his complaint after Dr. Kidd's explanation, has rendered it necessary for me to make these observations; but I confess I cannot see any sound basis for these complaints of Dr. Kennedy, inasmuch as the mode of debate adopted was more likely than any other to effect the object he had in view—to sift the subject to its utmost, and thereby to elicit correct

conclusions and arrive at the truth. A hasty debate, with crude and indigested views could not have effected this object, whilst giving every individual who desired it an opportunity of putting his views on paper was manifestly the most likely way of effectually attaining this result. For these reasons, therefore, I should have taken, as I have already said, the whole responsibility of the matter upon myself, even if the Council had not adopted the course indicated; and I cannot but think that Dr. Kennedy ought to have been the advocate for, rather than the complainant at, its adoption.

There is another point with regard to which I feel called on as President to make some remarks. In a comment on our debate in one of our valued medical periodicals it is stated:—"In the first place the discussion has seemed to us to take the form rather of an overwhelming organization for the defence of individual hospitals, and for the extinction of Dr. Kennedy, who has ventured to doubt their usefulness, than a debate on a great sanitary question;" and again, "But we fear that an opportunity has been afforded to cavillers to say that the unanimity of the attack is so suspicious as to invalidate the deductions which may be made from the debate." I will not make the least comment on the phrases "overwhelming organization," and "unanimity of the attack," but will simply suggest that *unanimity of defence* would have been the more correct phrase. It must be borne in mind that rumours have been for a lengthened period afloat and conjectures rife throughout the city that a withdrawal, or at least a revision of the Dublin Hospital Grants was in contemplation, and that, coincident with those rumours, Dr. Kennedy's paper was submitted to the consideration of this Society. Finding then that that paper contained a formidable bill of indictment against the two Lying-in Hospitals of Dublin, it was, I conceive, the bounden duty of their officers, and of those who had been at any time connected with them, and who still felt an interest in their welfare, to come forward to disprove, as far as in them lay, the grave charges contained in that paper, and to demonstrate that his conclusions were founded on an unsound basis, with the view, not merely that a right verdict—the truth, the whole truth, and nothing but the truth—should be arrived at, but also that no data should remain uncontraverted which could on any future occasion or in any manner be converted into a weapon of offence by any individual or party hostile to the interests of these institutions. That the time selected by Dr. Kennedy for submitting his views to this Society was inopportune, must, I think, be freely admitted, but it must not at the same time be forgotten that he had more than two years ago enunciated almost identical views in his celebrated letter to the governors of the Dublin Lying-in Hospital. For myself I feel persuaded that his sole object has ever been to have redressed what appeared to him to be grave evils seriously impairing the usefulness of those institutions, and to have removed what

seemed to him to be cause for cavil or complaint against their arrangement, and that thereby these hospitals—more especially that one, whose well-being he has anxiously watched for so many years—might be enabled more perfectly to fulfil the objects for which they were founded, namely, the preservation of human life and the relief of human suffering.

Having made these observations, I will now refer to the paper itself, and whilst dissenting from many of the views expressed therein, I shall, as I have already stated, owing to the lateness of the evening, and the length to which the debate has already extended, confine my remarks to three material points treated in it.

The first of these refers to the treatment of puerperal fever. It will be in your recollection that Dr. Kennedy advocates the invariable abstraction of blood; he says, "In true puerperal fever of the present day the use of the lancet is rarely admissible; local depletion must be our sheet anchor. But on having recourse to it the secret of success is to reduce the pain by repetition of leeching, before reaction has had time to establish itself;" and again, "If he (the physician), find that his first leeching has not enabled the patient to bear pressure, and relieved her from pain, he should at once apply more leeches before reaction is established, and so on as long as the pulse will indicate the propriety of further abstraction of blood."

Again, Dr. Kennedy appears to have implicit faith in the use of mercury, as well for the prevention as the cure of metria. "There is no doubt," he says, "of its efficacy if you can produce its specific effects;" and subsequently adds, "Where peritonitis or metria were to be anticipated, as when the disease was prevalent, after manual interference in labour, or for removal of placenta, my habit was to commence at once, after the labour was completed, with small and frequently repeated doses of mercury, say a grain or half a grain of grey powder every third or fourth hour, and even applying, in more suspicious cases, mercurial ointment to the armpits and groins. By these means, without disturbing the patient's system to any serious extent, in the course of forty-eight or fifty-six hours a slight mercurialization may be perceptible, often before the time at which the disease could grapple the patient, and then should metria or inflammatory symptoms show themselves, by pressing mercurials a little more freely, the system may speedily be brought under its specific influence, and the disease may be thus easily checked, if it had not already been prevented."

Upon the views set forth in the preceding quotations I am altogether at issue with Dr. Kennedy, and I should be sorry indeed that the treatment therein laid down should go forth to the world as that of the Dublin School of Midwifery for the metria of the present day, or, in fact, for that of the last quarter of a century. The subject has, however, been

most ably handled by Drs. Denham, Madden, Sawyer, and Byrne, and it only remains for me to express my individual opinion upon it.

When Dr. Kennedy was in charge of the Dublin Lying-in Hospital no doubt the disease presented a purely sthenic character; but who that has been in practice here during the last quarter of a century or more has not observed that a change has taken place in the type, not alone of this disease, but of every other with which we are acquainted; that the sthenic form has almost ceased to have an existence, and that the asthenic is that we almost invariably have to encounter. It is true that at the commencement of an attack symptoms of a sthenic character may be met with; but who that has had much experience of puerperal fever has not noted that it degenerates with fearful rapidity into the lowest and most asthenic form of the disease; in fact, Dr. Kennedy strongly presses this very point on us; he says the disease "is a *poison fever*, and no matter how severely it sets in with local distress and anorexia, that it is essentially a disease of debility, passing more or less rapidly into a state of collapse." "Consequently," he adds, "we must look steadily forward and provide for the coming issue—collapse and sinking from exhaustion."

Whilst, then, I entirely endorse his views, that "our treatment must be directed to that, and nutritious broths, jellies, milk, and farinaceous food should be as freely given as possible compatible with their being retained on the stomach," and that "the same observations apply to stimulants," I cannot but maintain that in this disease, as we now have it, in the words of an old and valued practitioner, now no longer amongst us, we ought not to "spill the purple stream of life," but should from the very outset, no matter how inflammatory the symptoms may appear to be, husband the strength and resources of the patient for the crisis which is sure to come.

The depressing effect of mercury renders it likewise, in my opinion, in a great measure, inapplicable, except rarely, and then only in alterative doses. The very facts, which are well known to practitioners, and are fully admitted by Dr. Kennedy himself, that "in the worst cases there is not time for this (the specific action), and, indeed, the system seems insusceptible;" that he has "on one or two occasions seen puerperal fever prove fatal after mercurialization showed itself, and in them it was incomplete;" that he has "often seen mercury in every dose fail in producing the slightest approach to its specific effects;" and that he has seen "a woman who came in under the effect of mercury" "attacked with metria on the third day, and the attack proved fatal;" fully attest that in some of the more severe cases the drug is altogether powerless, whilst in many I feel no doubt but that it is absolutely injurious.

The treatment in which alone I have confidence, based now on a

very lengthened experience, is the early employment of turpentine, both internally in repeated doses of not less than a drachm each, and externally in the form of warm fomentations, the steady use of opium in grain doses every third hour, together with the free but guarded administration of quinine and iron, and the judicious employment of wine, brandy, and carefully-prepared nourishment. I may add that this treatment is that which has been invariably adopted by myself and my colleagues at the Coombe Hospital for many years; is, moreover, I believe, that of the Dublin School, and is more consistent with the pathology and nature of the disease we are now accustomed to meet than the depletion and mercurialization recommended by Dr. Kennedy.

The second point to which I would direct your attention is the vital issue raised in Dr. Kennedy's paper. He has therein almost designated our hospitals as pest-houses, and denounced them as being the cause of the death of their patients. "Be it recollected," he says, "these poor women flock to these hospitals under the impression that they are gaining a safe asylum in their hour of trial and distress; little do they imagine that they are, in their ignorance, taking a step that adds to their risk of death in a ratio, at the very lowest calculation, of 3 to 1, and the highest at 20 to 1, against their lives." He again says:—"The mortality from childbirth exclusively in the Dublin and Coombe Lying-in Hospitals combined amounts to 1 in 170 from 1864 to 1868; whereas those in the metropolitan district for the same period only amounted to 1 in 260." Again:—"In the Coombe Hospital 3 out of 4 die (on the present calculation) that should not, which calculation extends over a period of the last seven years, and the deaths ought to be reduced to one-fourth of their present number." "In the Dublin Lying-in Hospital 8 out of 9 die who should not, or the deaths ought to be reduced to one-ninth of their present number."

The gravity of the charge included in the preceding quotations can scarcely be exaggerated; but I must here ask has Dr. Kennedy proved that issue? When I heard that able paper read by Dr. Kennedy, I confess that it at first made a very strong impression on me; but a careful analysis of his facts and figures, and a review of the powerful arguments subsequently adduced against his deductions, have convinced me that but one answer can be given to that question—namely, that he has not.

The admirable reasoning of the several speakers, more especially the conclusive calculations and exhaustive arguments of Dr. M'Clintock, have left little for me to add, and have rendered it almost unnecessary for me to discuss the subject further. It may, therefore, appear to be a work of supererogation on my part to question any of Dr. Kennedy's figures or arguments; I shall consequently limit my observations to his statement affecting the Coombe Hospital—that 3 out of 4 (or 75 per cent. of the whole mortality) die that should not. I confess that this

statement of Dr. Kennedy's seems to me to be an arithmetical puzzle. I know not by what mode of calculation he has come to this conclusion—he gives us no clue to it; certainly it cannot be based on the return for the last seven years, furnished to him at his request; and the tabular statement subsequently submitted to the Society by Dr. Kidd in no wise corroborates it. During the 15 years included in that return, 89 patients died from all causes; of these 55 succumbed to metria, being only at the rate of 62 per cent., and even the addition of the 15 patients labouring under some form of puerperal fever—who were removed to other hospitals, and of whose fate we are ignorant, but whose deaths are here assumed for argument sake—would raise the death-rate from metria to but 67 per cent. of the whole mortality. If the calculation is made on the returns of the last eight years of that period—namely, from 1861 to 1868, inclusive, wherein the deaths from all causes, as shown in the return submitted to Dr. Sawyer, amount to 54, of which 30 are attributable to metria, the death-rate therefrom amounts to but 55 per cent. In estimating the value of these relative proportions, however, it must be remembered that these numbers respectively include the deaths from puerperal fever arising not merely from contagion, but also from every other possible cause. In order, therefore, to ascertain the death-rate of an hospital from metria acquired by contagion, we must, I maintain, first eliminate every case arising from epidemic causes, from contamination antecedent to labour, from contamination through the medium of attendants engaged in dissections, *post mortems*, or the practice of general hospitals wherein infectious diseases are under treatment, from operations or manual interference, or mental depression; in fine, from any and every cause that may be designated individual. When this is done the number from contagion will be reduced to a very small number indeed; and it is only this number with which hospital management can be in any wise charged, and which will bear to the fullest extent comparison with the 75 per cent. mortality attributed to contagion in Dr. Kennedy's paper.

Perhaps, however, I have misunderstood Dr. Kennedy's intention as to the application of his calculation. His words appear to indicate that of the deaths from all *causes* which result in the Coombe Hospital, 3 out of 4 die that should not, but the meaning intended to be conveyed may be that of the deaths *which result from metria* the 75 per cent. referred to emanate from preventable causes. If this be so, let us see how far it will stand the test of calculation. It will be seen on reference to the return of the Coombe Hospital for the last eight years, '61 to '68, furnished by Dr. Sawyer, the number of deaths from some one or other of the several varieties of metria, amounted to 30; or, if limited to the seven years from '62 to '68, included in the statement forwarded to Dr. Kennedy at his request, to 28. Of these, whether taking the eight

or the seven years' period, there are attributable to individual causes 16[a] cases, irrespective of 4[a] suffering from all the mental depression of seduction; thus the number is reduced in the one case to 10, and in the other to 8—being at the rate of 33 per cent. in the former instance, and of 28 per cent. in the latter; and it must not be forgotten that these respective numbers of 10 and 8 may be still further very considerably diminished by the abstraction of all cases wherein the disease emanated from epidemic or other causes already referred to.

On whichever, then, of the foregoing hypotheses we form our calculations, the conviction, I conceive, must force itself upon us, that Dr. Kennedy's calculations in the case of the Coombe Hospital are based on error, that his conclusions are consequently wrong, and his allegations incorrect. In combating the figures of Dr. Kennedy it will be perceived that I have confined myself to an analysis of the calculation he has applied to that one institution. I have done so—first, because of my own direct connexion with it for nearly twenty-eight years, but specially because the tabular statement of the deaths therein during the past eight years, furnished by Dr. Sawyer, so completely details the individual history of the respective cases, that a means is afforded of ascertaining, in the great majority of instances, the exciting cause of the disease which terminated the existence of each. The fallacy, then, of his calculations in regard to one of the institutions subject to his strictures being demonstrated—*ex uno disce omnes*—I apprehend that any conclusion regarding others, which is founded on the same, or a similar mode of calculation, must of necessity be equally erroneous, and must, in like manner, fall to the ground.

But why should I weary you with any reference whatever to figures? Dr. Kennedy himself has thrown them overboard. He, it will be recollected, has based the conclusions he has come to almost exclusively on statistics, and yet it is a strange fact that, when called upon to reply on the first evening of this debate, he said he had no faith in statistics, rejecting them *in toto*, excepting those of two or three institutions which

[a] 4 Forceps. Nos. 9, 20, 28, 54.
3 Version. Nos. 31, 43, 48.
2 Craniotomy. Nos. 17, 23.
2 Cephalotripsy. Nos. 44, 46.
1 Evisceration. No. 47.
1 After accidental hemorrhage of fourteen days' duration. No. 24.
1 After partial placenta previa. No. 45.
1 After adherent placenta. No. 42.
1 A confirmed epileptic. No. 32.

16
4 Unmarried. Nos. 8, 13, 33, 38.

20 Total.

appeared to carry out his own views. By so doing he at once swept away the very foundation of his argument, and left but a baseless fabric behind; the superstructure of his conclusions having thus fallen to the ground, no effort was necessary on the part of those who dissented from his views to overthrow them.

In maintaining his views, Dr. Kennedy has drawn comparisons between hospital and private practice; between the results of large metropolitan and small provincial hospital treatment; and between hospital and extern maternity labours; but to my mind, comparisons are perfectly valueless unless when made between like and like—when all the circumstances on both sides are identical, and the conditions in all respects similar. This, however, is not the case with Dr. Kennedy's comparisons, no similarity whatever existing in any of these respective cases. Who, I ask, would think of comparing the poor with the wealthy—the victims of seduction with the virtuous—those who are below par in every sense of the word, mentally, morally, and physically, with the healthy—those, in fact, who live their whole lives in "poverty, sickness, and dirt," with those who are possessed of every comfort and luxury? Further, I would ask, who would compare an agricultural population with that which is to be found in the purlieus of a city; or the healthy population of a small provincial town with the inhabitants of a metropolis? and yet Dr. Kennedy has to a considerable extent done this.

No doubt, Dr. Kennedy admits that the mortality within a lying-in hospital should be in excess of that in extern maternity practice, inasmuch as the former "admits the worst cases, many of them advanced in labour, and sent from considerable distances, exhausted and half-starved," and that "these matters must be taken into account as increasing the mortality;" but when it is considered that these institutions have, in addition, to bear the burden of innumerable cases reduced to the most deplorable condition from previous neglect and unskilful treatment, often from actual mal-practice, and have further to afford shelter to many who fly to a crowded city to hide their shame, from country districts, which are thereby relieved from the effect their deaths would produce on the local statistics, the only wonder appears to me to be, not that the mortality of metropolitan hospitals is so large, but that it is relatively so small—not through the intervention of contagion, but from what has been already designated individual causes.

I may here, perhaps, be permitted, parenthetically, to express my opinion on the relative value of the two respective systems—the intern and extern treatment of midwifery cases. Each, in my opinion, has its advantages and its disadvantages, each its merits and demerits. In the one a life may be casually lost through the medium of contagion—ten in eight years as a maximum, as already shown in the Coombe Lying-in

Hospital—but many lives are saved through its intervention, which, in all probability, would have been lost outside owing to the want of proper treatment, constant care, and suitable nourishment; and in the other, the anxieties resulting from absence from home, the thoughts of a husband neglected and children uncared for, retards convalescence and militates against recovery. In fine, one is of incalculable value to some claimants for relief, to many the other alone is available. These considerations, and an experience of both systems for more than a quarter of a century, have led me to the conviction that each is a necessity, and that the union of the two in the one institution is the only arrangement compatible with the exigencies of our metropolitan population. Such is the arrangement now for a considerable time carried out in the Dublin Lying-in Hospital, and such has ever been since its foundation that of the Coombe Lying-in Hospital.

I must now hasten to the third point to which I purpose to refer—namely, the remedy Dr. Kennedy proposes to meet the evil he has depicted in his paper—"grouped but isolated cottage or pavilion hospitals, with only one, or at most two beds in each isolated room." I believe few will deny that metria is communicable by contagion. I, myself, am a believer to a limited extent in the contagion theory; but, as I have already shown, the whole number attacked through its intervention is very small indeed; yet, small though it be, it is the bounden duty of every hospital physician to adopt any measures likely to prove preventive to the progress of this dreadful scourge; but has Dr. Kennedy suggested the right remedy? I do not think he has. The deduction drawn from the statement of Dr. Stokes, that puerperal fever prevails to a frightful extent at Ballarat, where a cottage system of dwellings exclusively prevails, is altogether against Dr. Kennedy's theory. But even though this fact did not exist, I conceive that his plan would be wholly impracticable on financial grounds, and inexpedient for moral reasons. It would be impracticable because of the enormous expense necessary to maintain such an establishment; and I apprehend that it would be utterly impossible for any institution in Ireland—even the Dublin Lying-in Hospital, with all its funds, or perhaps I should rather say with all its poverty, for its resources are, I believe, scarcely adequate for its proper maintenance, even in its present condition—to meet the outlay incidental to such an arrangement, entailing an enormously increased staff. But, I believe, Dr. Kennedy's plan is also open to objection on moral grounds, and this topic has been touched upon by at least one speaker. If the hospital was constituted of a large number of these cottages, each occupied by its nurse, its ward-maid, and perhaps its pupil midwife, I would defy any proper supervision being exercised over it, and I greatly fear that demoralization would be the necessary result, and that the whole establishment would degenerate into a "wren's nest," or, as has been

suggested by a previous speaker, would be nothing better than a rabbit warren. In fact, I am not at all sure but that an institution originally intended for the relief of lying-in women would, after a time, become one for the creation of lying-in women.

But while we decry one remedy, we are not justified in omitting to seek another, so long as we admit that even a single life may be sacrificed to contagion. I believe the true prevention is isolation—instant and complete isolation of the infected from the healthy. Any of us actively engaged in practice, either private or hospital, must be aware that contagion does not exist in the incipience of the attack. If then we remove suspicious cases, not merely from the ward, but from the building itself, to another altogether apart and separate therefrom, totally excluding all possible intercourse between the occupants of the two branches immediately on the appearance of a threatening symptom, and before contagion has become possible, we obviate all likelihood of the disease extending through the medium of contagion transmitted by atmospheric or personal channels. This is the view which Dr. Kidd has already pressed upon your notice, and which has guided us in the arrangement of the new wing now in course of erection at the Coombe Hospital, which is to be, as he has already informed you, a distinct building, isolated from that now in existence, and the top story of which—to be approached by a stair for its exclusive use—is to be appropriated for the reception of cases of contagious disease occurring in the lying-in wards of the hospital itself; and I have no doubt but that a similar arrangement could be easily and economically effected in the Dublin Lying-in Hospital.

I had intended to have said more on these interesting topics, but the lateness of the hour warns me to bring my observations to a close; I shall, therefore, in conclusion, take the opportunity of thanking the several members of the Society, and our visitors who have taken a part in the debate, for the good tone and temper which have characterized it throughout. There may have been a casual expression dropped which had better been omitted, or some facetiæ given utterance to which might have been dispensed with; but the whole character of the discussion has been such as, in my mind, to raise the Dublin Obstetrical Society in the estimation of the profession. I have also to return my most sincere acknowledgements to Dr. Kennedy for the great care and trouble he has taken in the preparation of the able and remarkable paper he has submitted to our attention, and for the tact and ability with which he has developed his views; and I feel that I am only expressing the wishes of the Society in thanking him for being the means of eliciting so admirable and able a discussion.

It only now remains for me to announce that the Society will again meet to receive Dr. Kennedy's reply on the whole debate.—*June 5th*, 1869.

On July 10, Dr. Evory Kennedy replied as follows :—

Sir, in answering the criticisms on my paper, I must, in the first place, say I was not ignorant of the effect likely to be produced by grappling with an evil such as I felt it my imperative duty to expose, and, by the weight of public opinion, if possible, to remedy.

Neither was I ignorant of the fate that usually attends reformers.

I was warned on all sides not to bring a hornet's nest upon me, and cautioned not to disturb the equanimity of the physicians of Dublin by proposing impracticable changes in their darling institutions, hitherto the pride and boast of our city.

Sir, my motives forbade my giving ear to those timid counsels. My reliance upon the strength of my cause, which is yours alike, coupled with the estimate in which I hold the sound-thinking, liberal-minded, and philanthropic members of my profession, led me to the conviction that the profession, in this city, would never lend themselves to a feeble attempt to stifle or crush truth, if fairly and manfully brought before them.

It would have been easier for me to send my paper to the press, and avoid the ordeal of a *vive voce* discussion, with all its excitements, irritations, and personalities. But this plan would have failed in accomplishing the objects I had at heart, namely, eliciting fully all that could be said against as well as for the proposed changes; to meet objections fairly; and, further, to draw the attention of the whole profession to the importance of the subject, and what I considered the crying nature of the evil, as the best and speediest means of effecting its remedy.

From this it will be understood that I was quite prepared to expect an animated discussion, with much discrepancy of opinion. The mildest of our poets extolled the use of the *animated no!* and a discussion cannot proceed without it.

I am bound to say that this debate has proved no exception to the rule; for, although some important points have been quite overlooked, and although some statements have been repeated and reiterated, which, in my judgment, do not touch my arguments at all, or else misrepresent them, still I will candidly admit that my opinions and facts have been on the whole fairly and fully as well as freely discussed.

I purposely and carefully abstained from using personal allusions, or warring with individuals. My hostility was directed against a system for which no person, living or dead, could individually be held responsible. The system of large lying-in hospitals was in operation before the birth of the fathers of the present generation, and it was due, in a large measure, to the benevolent exertions of the Physicians of an earlier period.

The somewhat novel position in which I find myself placed of having to answer, at one and the same time, the objections and criticisms of sixteen learned doctors poured upon me, "quick, thick, and heavy, like

a thunder shower," imposes on me a difficulty not easy to surmount, with due consideration of your time and of the task before me. Fortunately that task has been lightened by the fact that most of my commentators have answered themselves and each other. I was in hopes that what remained to be done by me might have been done by grouping my critics; but this I found impossible to do satisfactorily. I am forced, therefore, to take them separately. I shall do so in the order in which each of the seventeen gentlemen spoke.

Dr. Johnston, the present master of the Dublin Lying-in Hospital, began the debate, and I shall deal with the salient points of his paper. He says he would be *wrong* in *not* contradicting my assertion, that the intention of Moss in relieving the miseries of lying-in women has failed, because the exertions of its masters have not rendered it as healthy as one could almost desire.

But it is proved that the mortality of the hospital has been for the last fifteen years 1 in 31. Unless, then, he looks upon the death of so large a proportion of patients as Moss's intended means of relieving them from their miseries, it follows Dr. Johnston *was wrong* in contradicting me, and that he is equally *wrong* in his other conclusion, that the hospital has been rendered *as healthy as one could almost desire.*

Dr. Johnston says that he has succeeded in maintaining a perfectly pure and innocuous atmosphere in the lying-in hospital, and continues:— "I may *certainly* say that anything like a *miasm* or *poison* does *not exist*, and confidently affirm that nothing approaching to contagion has appeared." Yet afterwards Dr. Johnston answers his own assertions by saying: "No doubt we have had deaths from metria, but in almost all instances they were either victims of seduction, &c., or others labouring under distress of mind."

Dr. Johnston says my statements are based upon wrong hypotheses.

Negatur; the hypotheses are strictly correct; that is, if Dr. Johnston's objections deal with what may be regarded as bearing upon the hypotheses he alludes to. Let us take them in detail.

Dr. Johnston cannot see why puerperal fever should prevail in the lying-in hospital when fever, typhus and erysipelas, prevailed in medical and surgical hospitals.

Very possibly. Neither do I *quite*, although I fancy we shall all soon see. But I know by experience *it is a fact.* So did the late Dr. Collins and the late Abraham Collis; so did the late distinguished Dr. Joseph Clarke and Dr. Labatt.

Dr. Johnston can't conceive why, because it frequently happens that puerperal fevers follow the introduction of these zymotic diseases into the lying-in hospital, that hospital should be charged with being the habitat of such a poison, or that zymotic disease in any way engendered it.

I can't conceive how we can arrive at any other conclusion from the frequency of the one following the other than that they stand in the relation of cause and effect. Although the hospital is charged with being the habitat of puerperal fever—a fact which cannot be disproved—it is not so charged because it may be specially engendered by the introduction of a case of zymotic disease (see Propositions 4 and 5).

Dr. Johnston can't admit that puerperal fever prevails endemically in hospitals.

It must do so if the great hospital be its habitat; and this, although a self-evident fact, has been proved by me over and over again.

Dr. Johnston affirms it is not correct, in fact, unjust, to say that when puerperal fever appears it *generally* first shows itself in the great hospitals.

Not only is this the fact *generally*, but it is notorious that it often shows itself and commits dire havoc in our hospitals without extending epidemically or at all out of them (see Dr. Joseph Clarke's letter to Sir C. M. Clarke, *infra*).

Dr. Johnston adds, after dwelling upon the precautions and advantages of the hospitals as compared to the hovels of the poor:—"It is hardly, if at all, possible that malaria, or miasm, or any poison, could find a habitat in the hospitals."

But it does, and does to such an extent that the mortality in all the great lying-in hospitals in Europe, notwithstanding all the precautions adopted, has forced itself upon public and professional consideration, and we are here to investigate the causes of this *impossibility* according to Dr. Johnston.

Again, Dr. Johnston and his assistant deny the existence of a noxious atmosphere in the hospital during their period of connexion with it, or from May, 1868, to May, 1869. The latter gentleman asserts that the present death-rate is much lower than Dr. Kennedy's calculation, based on the average of the last fifteen years, leads him to think it is. Fortunately, or, rather, I should say, unfortunately, he answers this question so completely in disproving his own assertions that nothing more remains but to adduce his own statistics. Need I say that it is to me a matter of pain and regret, as a governor of the institution, my being obliged to prove that this gentleman's statistics do not confirm the statement of so marked an improvement as they led us to expect. His statistics for the year 1868 are that there were 1,132 deliveries and 29 deaths, but of these there were, as far as I can elicit, 4 deaths in the chronic ward, which are not to be taken into account. If I am in error in this deduction, it is not my fault, as the report did not, as it ought to have done, give us the exact number from all causes, metria, of course, included. But 25 deaths in 1,132 leave a little over 1 death in 45. Now, in the fifteen years, from 1854 to 1868 inclusive, the fifteen years

for which I take my average as establishing endemic disease, there are six years in which the death-rate is more favourable to the hospital than that of last year, of which Dr. Johnston and his assistant speak so confidently of "*nothing* like miasm existing," in the very same breath that they admit 18 deaths by zymotic puerperal fever on the high average of 1 death in 63 from this disease alone. To prevent mistakes on this subject we shall give the death-rates for the seven years alluded to, bearing in mind that the gross death-rate of the last year by their table, errors excepted, is 1 in 45$\frac{1}{6}$. In 1854 the death-rate was 1 in 53; in 1856, 1 in 64; in 1857, 1 in 46; in 1859, 1 in 66; in 1860, 1 in 54; in 1864, 1 in 46; and in 1865, 1 in 44.

Although I have deemed it necessary, in order to draw a fair comparison with other tables of mortality, to deal with the gross mortality in discussing this question. I am perfectly satisfied to take the statement of the 18 cases of metria out of 1,132 deliveries as conclusive evidence that the disease still holds its habitat and haunts the wards of the Dublin Lying-in Hospital, the large mortality of 1 in 63 *by metria alone* amply confirming this statement and that upon the statistics furnished by the present resident assistant.

But, sir, I will candidly admit, had it been otherwise, and had the tables furnished from May, 1868, to May, 1869, afforded a different result, had the death-rate fallen to 1 in 66, nay to 1 in 100, or risen to its highest year (1862), 1 in 14, it would not in the slightest degree have affected the case I have made, or the necessity that exists for dealing with this important question. It did not require an occasional improvement in the mortality, extending, perchance, over a year or two, aye or three years, to convince those who have studied this disease philosphically and free from prejudice, that it is fitful and capricious in its appearance, disappearance, and duration. My two nights' efforts to elucidate the laws which regulate this disease must have proved nugatory if this, one of its universally recognized laws, has not been impressed upon you.

I have here selected the points of Dr. Johnston's speech which appear to require an answer. The rest of his obseravtions may be described as a eulogistic description of the management and advantages of the lying-in hospital, in which I should fully agree if its defects in construction were remedied. But with his conclusions, and with his arguments, meant to explain why it should be "as healthy as one could desire," and with his commendations of its construction, which I have pronouncod and maintain to be "faulty"—with these I disagree, simply because the experience of the hospital has proved that it is *unhealthy*, and that upon Dr. Johnston's own showing. For, surely, where all the other advantages are so great, and arrangements so perfect, the tenor of his own reasoning leaves me no alternative but to ascribe its unhealthiness to its *construction*, which I affirm, and Dr. Johnston thus proves to be "*faulty*."

Now, wherein lies this faultiness of construction? In its being a double house, with an intermediate corridor, converting 4 great wards, with 9 beds in each, and 8 smaller wards, with 3 beds in each, into 1 great warren or plateau of connected rooms, with a similar plateau over it, with perforated openings in the ceiling of the lower corridor, which secures the same atmosphere pervading both corridors and the chambers. A generally commingling atmosphere is thus secured through doors, staircases, and perforated openings, so connected throughout that miasm must pervade all, if once generated. In fact, so constructed that isolation is totally impossible, as it is impossible to open a door, or to go in or out of any room in the building, without diffusing the tainted atmosphere, if it exist, throughout the whole structure.

I submit, then, that my original statements are confirmed, and Dr. Johnston's objections answered in the most conclusive manner, by the statistics furnished of the present state of the hospital by his own assistant.

Dr. Kidd, whose views appear to me more advanced upon the subject under discussion, and to approach more nearly to the state in which experience has now landed us, than any other of my commentators, leaves very little at issue between us. In fact, after examining my tabulated analysis of the speaker's, criticizing my thirteen propositions, I find that, directly or indirectly, he agrees with most of the propositions, and as he is about to give the best practical test of this in the manner in which the new wings of the Coombe Lying-in Hospital are to be constructed, I sincerely deplore that he should be only *almost*, and not *altogether*, convinced of the necessity of complete isolation; for an error in this respect now, will perpetuate this deplorable and yet preventable mortality, as if the modified attempt at isolation, as recommended in the resolutions at the Paris conference in 1866, which he describes as about to be adopted in the Coombe Hospital, be carried out, it may help to delay the progress of this great question for an indefinite period. That it may lessen the mortality is quite possible, but that it will prevent metria I do not anticipate; and whichever it does, if it leave the impression on the public mind either that enough has been done in this experiment, or that nothing more need be attempted, as this has failed, then, I am justified in saying that the change contemplated, by its failure or partial success, only perpetuates the mortality.

Sir, I can use no language stronger in proof of this than Dr. Kidd's own. After admitting that the mortality of the Coombe, for the last 15 years, has been 1 in 65=730; and ascribing the occurrence of metria, to a large extent, to epidemic influences, he goes on to say—"But let it not be supposed, when he spoke in this way of individual causes of the disease, and of epidemic causes, he for one moment doubted the influence of contagion. On the contrary, he believed contagion was a most

influential cause of it, that the disease was highly contagious, and that if they were to prevent it, they could only hope to do so by directing their attention to this. Dr. Madden had told them just now, that he never saw two patients ill of puerperal fever in adjoining beds. They (*i.e.*, the Coombe Hospital) had one remarkable instance where they had a case of puerperal peritonitis, in which they thought it wise to apply leeches. This woman got erysipelas, and recovered, while the patients at each side died from puerperal fever, in one case, accompanied with effusion into the joints, and in the other, the patient died before that could have taken place." By a strange coincidence, Dr. Kidd's next sentence, and next branch of his subject, commences thus :—"The statistics of extern practice and of lying-in hospitals had been compared. If the results could be depended upon, lying-in hospitals ought to be closed at once and forever. '(Hear, hear.)'" After expressing his want of reliance in the statistics furnished by extern maternities, he continues :—"But, notwithstanding all this, he must admit that there was a large proportion of mortality due to the influence of the hospital. He believed it was impossible to collect a number of patients under one roof, and yet not to have an hospital atmosphere, and that that atmosphere was more or less injurious." Here, Sir, we have the honest manly statement of this enlightened man—himself an hospital physician of no slight experience—conversant with his subject, and sustaining, to the letter, the views I have propounded, on the eve of building an hospital so constructed as to collect a number of patients under one roof, when he admits fairly, "it is impossible to collect them under one roof and not to have an hospital atmosphere, and that that hospital is more or less injurious." Is it to be wondered that I should, under these circumstances, implore him and his colleagues publicly, as I have already done in private, at least to hesitate in raising any structure that shall not render the isolation of parturients complete?

On the third night of our discussion, I made a few observations upon the papers that had been read that night; but, by some strange oversight the reporter employed by the Society to detail the proceedings omitted my observations upon the St. Petersburgh Hospital in reply to Dr. Kidd. Dr. Kidd observed, in reference to isolating parturients, "When Dr. Kennedy spoke of isolating patients he meant it literally by having a separate ward for each patient. The plan had been tried in the hospital of St. Petersburgh, which in 1852 had been reformed by the Grand Duchess Helena. It was increased by the erection of a new wing of the figure of the letter L on the corridor plan; opening off the corridor were a number of small wards in which the patients were placed. But what had been the result of that? The reform was instituted in 1852. The mortality for the seven years preceding it was 3·12, and for the seven years succeeding 2·93. That was to say before the patients were placed in

separate wards 1 out of 32·17 died, and that after this change—after they had undergone all this expense and trouble, and more or less injured the efficiency of the hospital, the only change was that 1 died in 34·09." Now, had the Doctor himself happened by any process of transformation to be one of the two lives saved, I doubt whether the *expense* and *trouble* would have stood in the way of his recommending the reform, inefficient as it was.

On this subject Dr. Mapother, no bad authority, writes to me, Dr. Kidd did not think the saving of 2 out of every 34 deaths worth the changes made in the St. Petersburgh Hospital. Sanitarians are proud of reducing a death-rate by 1 in 1,000. But, in my answer to Dr. Kidd on his statement, and which I complain of having been omitted, was based on the following extract taken from Dr. Oppert's volume on Hospitals and Infirmaries. In describing the Helena Hospital he writes thus—"The institution consists of an old part which was built more than thirty years ago, and a new one, the erection of which was patronized by the lady the name of whom the hospital bears at present."

"The whole has an irregular shape; the old part contains two floors, about fifty beds, the new one *three*, under one roof, not including cribs, can be placed in the wards. The old part has the shape of a letter L (reversed), and one portion contains a central corridor, into which small rooms open on each side, the other has one corridor running along the wards.

"In the new part, which has the form of an open quadrangle, the sick wards are small, for one or two inmates. Most of them contain open fire grates; there are tile stoves in some others. *The wards open into each other*, and are situated back to back on either side of the wall." And yet Dr. Kidd adduces this as an instance in which, as he himself expresses it, my plan of "isolating patients literally" was tried and failed. My answer simply is, that this is directly the *reverse* of my plan; that my plan has not been tested in the Helena Hospital, and consequently has not failed. Dr. Kidd might just as well have said, that the plan he and his confreres are about to carry out at the Coombe Hospital, of isolating or separating the patients *after* the attack of metria has shown itself, is my plan, and if it also fails, might hereafter affirm that in the Coombe my plan had been also tried and had failed. My plan recommended to certain parties in America who consulted me upwards of thirty years ago as to the best means of building to be adopted in the construction of lying-in hospitals, with a view to exclude metria, was that each building should be completely isolated, and with not more than two beds in each ward.

Dr. Mapother, in a few pertinent words, evinced how well he understood the subject of zymotic disease, upon which I may say, without flattery, he is "*facile princeps*," our highest authority in this kingdom. It affords me great satisfaction to know that I possess in this arduous struggle the

support and unbiassed disinterested approval of this enlightened physician, who has made zymotic disease, its causes, prevalence, and laws, his especial study, and whose writings and lectures have thrown so much light upon it. This liberal-minded physician, thoroughly versed in the use and value of statistics, has recorded his opinion, in the spirit of a juryman or expert, and one who attended this debate with no determination to support a foregone conclusion—no mistaken idea that his darling hospital was attacked and must be defended at all hazards, provided with no elaborated one-sided brief that occupied weeks in the preparation, and hours in the delivery, but, best of all, with no motive but to arrive at truth. And what did this indubitably competent, this unquestionably honest expert and juryman say? "He did not think one of the speakers had controverted the remarkable statistics which had been brought forward by Dr. Kennedy with regard to the mortality in the Dublin Lying-in Hospital, and that in small provincial towns, Waterford, Limerick and New Ross."

Sir, in dealing with the pertinent remarks of Dr. Churchill I feel bound to state that he has entered upon the consideration of the question at issue in a philosophic spirit, and that his observations have helped us much in the elucidation of this important subject. Nay more, I feel called upon to add, that however I may regret the insertion of the last paragraph of his paper, in which he pronounces "the changes I propose both *hasty* and *injudicious now*," yet the tenor of his reasoning, the negative-pregnant contained in the words not *now*, which mean, if anything, the necessity that shall or may exist hereafter, but above all, his table of statistics, and the admirable rules he has so strongly insisted upon, in their reading and application, render his observations not only valuable, but of the greatest value, in confirming all the views I have propounded and insisted upon in my paper.

We shall pass over his strictures upon the statement attributed to me as to the non-occurrence of metria in patients in their own homes, by simply referring him to the words "*comparatively rarely*," which he republishes in my ninth proposition. The oversight in which he, in common with Dr. M'Clintock, giving Dr. Joseph Clarke's death-rate by puerperal fever, places it at 6 not 3, as it ought to be, out of his 3,847 private patients, a rate which so nearly coincides with mine, being 1 in 1,282, and the mistake which ascribes this statement to Dr. M. Clarke, whereas it was contained in a letter written by the late Dr. Joseph Clarke to Sir Charles Mansfield Clarke, that we shall again have occasion to refer to.

Dr. Churchill, sir, asks a very pregnant question, and one the answer to which contains exactly what we are looking for. If metria be produced *spontaneously*, why do long periods of freedom from it occur under precisely the same hospital conditions as those which accompany its appearance? This question is seriously asked me by my friend Dr.

Churchill. Why, sir, the learned president of the College of Physicians might just as well have asked that unanswerable question that puzzled all the philosophers of the last century, and remains, as far as I am aware, still unanswered, "What is the weight of a mail coach?" I will ask him in return to substitute the adverb epidemically for spontaneously, and answer his own question, explaining why *epidemics* should recur under precisely the same circumstances, and it is not impossible, when he succeeds, he may apply the same solution to both questions.

Neither shall I dwell upon his views of the epidemic prevalence and contagious extension of metria views, so strongly confirmatory of what I have expressed. But I pass to his statistics. In the first instance, reiterating his own remark, "That to be of any authority they must be extensive," to which he added a more doubtful proposition, namely, "In fact their value is exactly in proportion to their extent." Had he substituted the words comprehensiveness and accuracy for the word extent I should have accepted the latter as freely as the former proposition. But possibly he intended to imply or include these ideas on the word extent, and in that meaning, therefore, I shall deal with it.

Dr. Churchill treats of three classes of statistics, giving the proportion of death-rates in each. The first includes the mortality of six obstetric practitioners, some giving the deaths by puerperal fever merely, the others the general mortality. Dr. Churchill justly remarks that they afford no sufficient ground for estimating the death-rate after confinement, and passes them without comment, a line in which I shall follow his example, merely remarking that, if we could hope by any exertion of ours to bring the death-rates in hospitals to any reasonable approximation to those detailed in his private practice tables, we should be but too happy.

The next class of statistics given by Dr. Churchill is that collecting the number of cases and proportion of deaths to deliveries occurring in the attendance upon out-door patients from the maternities—and here Dr. Churchill has his own principle of *extent* fairly carried out—he adduces no less than 236,665 cases as attended from six maternities, giving 1,590 deaths, or the proportion of 1 in 149 deaths by puerperal fever, and this he contrasts with 71,090 cases in large hospitals, giving 1,156 deaths, or 1 in 62—that is, he gives

Maternities death-rate, 1 in 149
Large hospital death-rate, 1 in 62.

Now, although I do not bind myself to the accuracy of these statistics, I accept them on the authority of Dr. Churchill as his basis of argument, and merely so far as they support his negative-pregnant that "*now* is not the time to provide small parochial hospitals," and ask is it nothing to save $2\frac{1}{4}$ to 1, on his own calculation, of all those women at present confined in the great lying-in hospitals? and if *now* is not the time to do this, when will

that time be, and what will be the additional unnecessary mortality that we shall then be answerable for when that time arrives? Simply repeating, that I am not answerable for any confusion Dr. Churchill may have fallen into in his attempt to give the mortality of puerperal fever distinct, as, finding myself the impossibility of doing this from the unreliability of the returns as well from some of the hospitals as maternities; in consequence of the irregular manner in which the different varieties of metria were reported, I adopted, as the only reliable test of comparison the total mortality in all cases, unless when I state the contrary. I should much have preferred giving the mortality by puerperal fever in all cases, with the gross mortality, had this been available and reliable.

As Dr. Churchill has not drawn the distinction between deaths by metria and the total deaths, it would not be fair by him, nor would this discussion derive any benefit by continuing the comparison, or dwelling upon his returns of the large hospitals. We shall, therefore, pass at once to his third table, that of what he terms the small hospitals, and in doing this must be allowed to repeat Dr. Churchill's axiom—"In comparing numbers we must take care that we are comparing like with like." Now, on this his own ground of comparison, I altogether reject this table of statistics, and deny that any ground of comparison holds. Dr. Churchill collects twelve *what he calls "small hospitals"* to make his comparisons by, and yet of these twelve *only three are* small hospitals, the other nine exhibiting all the vices of the larger lying-in hospitals, with none of their advantages. I feel convinced that this was an oversight on the part of my friend, or he never would have dreamt of drawing such a comparison as ranking these nine hospitals in the same category, or classing them as small or isolated cottage hospitals, such as I recommend. Nine out of twelve of these so-called small hospitals have annual deliveries varying from 400 to 140, and some of them contained as many as thirty and fifty beds. Several of them were old houses, with a number of rooms communicating, with story above story, and the patients huddled together, with every disadvantage of small crowded rooms, and a stagnating atmosphere. Why, compared to these the palatial edifices and well-ventilated spacious halls for dormitories, such as the hospital which is such a source of pride to us in our city, might even afford better chances to recovery to patients in their delivery.

Of the three cottage hospitals which I compared with the great hospitals, on the contrary, one had only 30 deliveries in the year, and the largest the Waterford had only 115, and these scattered over four rooms. Is it then to be wondered at, that in these three, the mortality only amounted to the average of 1 in 282; whereas deducting these three cottage hospitals from the twelve in which the mortality is given by Dr. Churchill as that of the small hospitals, I find the mortality

of the remaining nine amounts to 1 in 59. Why I doubt whether a stronger case could be made for the cottage or isolated hospitals, than that furnished in this table of Dr. Churchill, or, if properly investigated, that any stronger proof could be given of the accuracy of my propositions, No. 3 and 4, which asserts the mortality to exist in proportion to the number of parturient patients cohabiting and breathing the same atmosphere, and that metria finds its habitat where large numbers are delivered under the same roof. Propositions that imply number in proportion to space, as well as absolute number; and yet, these proportions have been cavelled at by almost all my critics. Thanks to Dr. Churchill, however, he has answered these unanswerably in this table.

But, we have one more matter to touch upon with my friend Dr. Churchill. He very properly insists in statistical calculations upon "the numbers being very large, and the time embraced in them being considerable." Very justly he remarks, "in a small hospital or private practice you may go on for years without a death, yet after the lapse of another year the average death-rate may be fearfully high, just because an epidemic intervened." Now, I apply this rule to his table, and I find that the first and second entries, Dr. Beatty and Dr. Churchill's hospital statistics, only embrace three years, and are, compared with the Waterford Hospital, embracing thirty. It is hard to say whether, if the statistical Kaleidescope to which we alluded on our first night's discussion had revolved for the other twenty-seven years, Dr. Churchill and I might not have agreed upon this point, as we do on most others, and thus the convictions of his own well stored mind, the result of years of ample experience and deep study, might have remained unchanged, and been applied in co-operating with me in this my arduous struggle for the benefit of our suffering fellow-creatures. Be this as it may, however, I would strongly urge upon him, when he is tempted to be led astray again by statistics, to hold fast by his own excellent maxim—"That in comparing numbers with numbers we must take care that we are comparing like with like."

Dr. Morgan's report of the mortality in childbirth in the Lock Hospital, 1 in 74, is extremely interesting in its bearings upon this discussion.

It is quite evident that the paucity of the births and the consequent segregation of the parturient element was a main cause of the comparative absence of metria in his cases. But, on the other hand, we had the parturient exposed to what we laid down as a mischievous influence in her inhaling the atmosphere of a surgical hospital, an element that should have increased the mortality, as we have found it do, in the foreign hospitals in which surgical cases and deliveries are congregated, unless some corrective principle was in operation.

We imagine we have exactly met a principle in the theory, old as the fathers in medicine themselves, namely, that morbid poisons cannot co-exist in the same individual. Here the syphilitic virus was in occupation and resisted the metria zymoticene.

This theory I am quite aware admits of some apparent exceptions, exhibiting modifications in the types of disease, depending upon intercurrent poisons; but here again the common original type may be the explanation.

I have, myself, witnessed strange struggles for the mastery in cases, where two poisons had found a nidus in the same individual, at the same moment.

One of the most remarkable was a lady under my care about two years since. Her children were ill with mumps, and she sickened with fever and stiff jaws, but without the glands assuming the usual enlargement. The fever, however, continued, and with considerable, in fact, unwonted severity, causing me some anxiety, and after four days, a crop of measles, with all the usual accompaniments, appeared over the body, ran its course, and subsided on the fourth day; but the fever kept up. Difficulty of swallowing returned, the parotid and submaxillary glands became very much enlarged, and the case then ran the usual course of mumps.

Dr. Morgan's observation upon the distress of mind necessarily existing in the deplorable cases of labour in the Lock are of much value.

Whilst I freely admit the influence of mental depression in causing bad recoveries in childbirth, and even predisposing to puerperal fever, I am of opinion that its influence in the latter respect has been overestimated in the observations that have fallen from several of the speakers upon this subject, but the very interesting report furnished by Dr. Morgan, of the Lock Hospital, and the South Dublin Union report, appear almost conclusive on this subject. What combination of conditions could be more calculated to produce mental depression than those of poverty, destitution, and syphilitic taint in the married female? but when we couple with these the frequency of seduction and prostitution as additional ingredients, we can scarcely conceive any influences more depressing to the human mind; yet, with this combination of depressing influences, the mortality in Dr. Morgan's cases in the Lock, with a delivery ward containing only *two beds*, and therefore never overcrowded, was only 1 in 74; whereas, in the Lying-in Hospital, with its mixed occupants and incidental cases of seduction, it has been 1 in 31½. The only assignable cause of the excess being the proportion of the deliveries under the same roof, constituting the hospital the habitat of metria in the one case; whereas, every other ingredient of calculation, mental as well as bodily, was in favour of the excess in the other, but it was wanting in the habitat.

The South Dublin poorhouse furnishes us with a parallel nearly but not quite as strong as this.

Sir, I was rather astonished when I heard it put forward by some of my critics, that the mortality in the extern cases attended from the

Dublin Lying-in Hospital amounted to 1 in 61 deliveries. I was aware that this branch of the institution had not been much encouraged, neither had the records of cases been very regularly kept. As a governor of the hospital I drew, about two years since, attention to the necessity of encouraging the out-door attendances and of keeping a more accurate record. I therefore applied to the Registrar for a report of the last year's deliveries, in order to compare them with Dr. Johnston's year of the hospital, when it was, in his words, as healthy as one could almost desire, and find by it that in the year ending March 31st, 1869, the extern attendances amounted to 147, with but 1 death. The mortality within the hospital for the same time amounted, as we have seen, to 1 in 45, or three and one-fourth to one of that without the hospital; and be it recollected, that this is the year in which the improved condition of the hospital, commensurate with Dr. Johnston's mastership, has been dwelt upon. Would we could even effect this saving within its walls.

We now arrive at Dr. M'Clintock's strictures upon my paper, and as this gentleman occupied an hour and forty minutes in developing them, you must bear with me as patiently as you can in the time required for their refutation. Our time will not, however, I trust, be misspent, as, from his numerous objections, his ingenuity, and his peculiarly pungent and asseverative manner of attack, I am quite aware that his observations produced a marked effect upon his hearers. I shall not follow his example, however telling as it may have been, of assuring you before doing it that I am about to annihilate him for ever. I shall not seek to forestall your plaudits by declaring my intentions of conquest which, like those of the boastful opponent of Tekeli, may fail to be realized in presence of the adversary. But I shall ask your calm attention to my reply, free of all favour, affection, and prejudice; as, in replying to this gentleman's somewhat bitter attack I shall be also meeting the objections of several others of my critics, whose arguments he has repeated and amplified. It is therefore, I repeat, the more necessary that a patient, considerate, and candid hearing may be accorded to me.

The spirit in which Dr. M'Clintock approaches the great subject that engrosses us (the means of reducing the admitted mortality in our lying-in hospitals) will be best understood by introducing him to you as the propounder of a novel and startling principle applicable to lying-in hospitals, and one which he enunciates in the language of Drs. Bristow and Holmes, "that a high death-rate indicates, as a rule, that an hospital fulfils efficiently the purposes for which it was designed, and that a low death, on the other hand, indicates, *cæteris paribus*, comparative inefficiency." Why, sir, this principle, even as applied to general hospitals, admits of great question; but its application to lying-in hospitals I shall not trust myself to characterize. Such an assertion might be made in the heat of debate, but furnishes no excuse for a gentleman in

his calm moments deliberately putting forth such a maxim as this before a society of enlightened physicians, namely, that "the principle enunciated (in these quotations) applied with little qualification to maternity as well as surgical or medical hospitals." We shall hear presently what Dr. Stokes says on the subject of labour as a natural process. How the learned Regius Professor must have shrunk, horror-stricken, from the avowal of such a principle!

After enunciating this paradoxical and shocking dogma, Dr. M'Clintock wanders off to descant upon reliable estimates of mortality in women in their own homes. But we shall not require him to go out of the four walls of the hospital in which he resided for ten years, to prove the case against himself. We shall not require to compare hospital with private practice to refute it. Let us take any period for which the hospital was free from metria, for a sufficient time, to give a fair average of deaths by the accidents of labour and diseases, independent of metria. Say the last three years of Dr. Collins's, and the first year of my mastership; that is four years, from 1830 to '33 inclusive. In these four years there were 8,844 deliveries and 48 deaths, or 12 annually; that gives 1 death in 192. Now let us take, say the last four years of the hospital, from 1865 to 1868 inclusive. The deliveries were 4,574, the deaths 149; that gives 1 death in say 30⅔. The result of the comparison of the hospital with metria against the hospital without metria is to give 5½ unnecessary deaths out of every 6 that occurred in the last four years, as compared with the period 1830 to 1833. Now, does Dr. M'Clintock mean to tell this intelligent Society that that excess of death-rate in the same hospital, under identically similar circumstances in every respect save one, the presence of metria, furnishes us "with grounds to draw his inference, and satisfy him with its foundation." "That a high death-rate indicates as a rule that the Dublin Lying-in Hospital fulfils efficiently the purposes for which it was designed, and that the low death-rate of Dr. Collins's four years, on the other, indicates (*ceteris paribus*) its comparative inefficiency in his period?"

Dr. M'Clintock triumphantly collects and recapitulates, what I had already given in my tables, the instances in the Lying-in Hospital in which the largest number of patients that had been delivered with the smallest death-rate; and after dwelling upon these as disproving my third proposition, he concludes by alluding to Dr. Labatt's most crowded period (the very one I had adduced as confirming my position) in these remarkable words:—"This was certainly enormous cohabitation, *and it did certainly breed an epidemic puerperal fever*, but there were three years of gestation before it appeared." Here we have the one grain of wheat in the bushel of chaff. But Dr. M'Clintock is in error in saying there were three years' freedom from puerperal fever at this period—there were only two, 1815 and 1816, and these he should have mentioned (a fact

that he, as master, could not have been ignorant of) were the very two years that the auxiliary hospital was added, and the crowding lessened. In 1817 the mortality rose again from 1 in 182 to 1 in 108, and in the next year to 1 in 62, whilst in 1814 it had been 1 in 100, and in 1813 1 in 40.

After such an admission, which common sense and common honesty extorted at Dr. M'Clintock's hands, followed by my commentary of the addition made to the hospital, which he suppressed, what becomes of his statistics collected from my tables with such assiduity, and his cavils as to the comparative mortality in New Ross and Waterford cottage hospitals. Sir, Dr. M'Clintock might possibly have better consulted the interests of our common humanity, as he certainly would have done his own consistency and good taste, if he omitted his charges of *exaggeration* and *unsoundness*, dealt so unhesitatingly against me, in the very same sentence in which he admitted that I had made a *strong* case against larger hospitals in my mode of dealing with them, and when he still further "*conceded readily* a greater mortality amongst women confined in these institutions as compared with the aggregate of women confined in their own homes."

Why these two points include the whole principle for which I have been contending.

My third and fourth propositions are the undeniable logical corollaries based upon these. Our eight nights have been occupied in testing the accuracy of these two propositions; propositions which, in slightly altered language, Dr. M'Clintock himself admits, and, as we shall see afterwards, explains; and yet, with rare inconsistency, whilst he admits the truth of my propositions, he occupies an hour and forty minutes of the valuable time of this Society, engrossed with a vital and all-important question, upon which so much depends, and in which our profession is as much upon its trial as are the lives of those women he admits to be sacrified; he occupies, I say, this Society for an hour and forty minutes, whilst confirming my position, in trying to detect flaws in my arguments, in dwelling upon accidental slips of the pen, in the transcription of my figures, and in magnifying them into grave errors of calculation. But, excuse me, sir, I am imitating his bad example, and forestalling statements to come.

But, it has been attempted by Dr. M'Clintock, Dr. Atthill, &c., with more ingenuity than candour, holding me to the strict letter of my proposition, that the mortality was in direct proportion to the number of parturients in a given space, to prove that I had asserted that that general statement applied *ever* and *always* to the *very time* at which that crowding was observed. I did not, however, make any such statement. On the contrary, the whole tenor of my reasoning goes to prove the reverse. I dwelt especially on the capricious nature of this zymotic

disease, and referred to the statistics of the lying-in hospital as showing this peculiarity in its endemic hauntings. But I went a great deal farther. I showed in distinct, unmistakable language what was intended in this proposition. Yet, strange to say, every gentleman who has attacked this Redan proposition has found it convenient to ignore these facts.

Page 293 of my paper states:—"If you will cast your eye along the Dublin Lying-in Hospital table there furnished, it will give you, at a glance, the longest duration of these endemic hauntings to which the hospital has been liable since its foundation. Observe where there are two figures in place of three, you will thus arrive at a rough result." Now, mark me, gentlemen. "From 1761 to '65 was one of these; again from 1767 to '70, and from 1772 to '74. From this year down to 1800 we meet only an occasional unhealthy year, but it remains at two figures until 1804, and so on, with only an occasional unhealthy year until 1818, the *most crowded state the hospital ever was in; in that year there were* 3,539 *deliveries. For three years the mortality then arose to* 1 *in* 44 *; in* 1826 *it arose again, and continued for the next four years at* 1 *in* 50." If language means anything, it will be evident from this, that I state that the mortality arose and continued as the result of the previous crowding. It will be perceived this convincing evidence of the truth of the sixth proposition has been inserted in italics, to prevent its again escaping the notice of my critics. Sir, I feel called upon to do this, and to dwell thus on this quotation, because, although I pointed out this passage and these figures of Dr. Labatt's mastership to Dr. Atthill on the first night of discussion, as refuting his assertion and confirming my proposition in the most conclusive manner, other gentlemen have followed his bad example, and ignored this passage in treating of this subject.

Had my commentators taken the trouble of examining the history of the hospital, they would have found, however, as above alluded to, an especial explanation of the delay in the outburst of the disease until the year 1817, although the numbers had been increasing from 1815 to 1818 by nearly 1,000 deliveries; it was this. In the year 1815 the crowding was checked by the purchase of what had been the hospital for the blind, which was then added to the institution. This lessened the crowding. It partially isolated, by separating the patients, and thus the outburst of the disease was delayed for two years. Not only this, but the death-rate absolutely fell from 1 in 100, where it stood in 1814, to 1 in 180 in 1815, and to 1 in 182 in 1816.

What could be more convincing, then, than this period of 1818 in confirming the relations between crowding and death-rate insisted upon by me?

Drs. M'Clintock, Atthill, and their friends oblige me to analyse

in detail the saliently marked periods which show the mere relation between increase, diminution, and death-rates in the hospital, as they have done, a not uncommon thing with young philosophers, they have confounded cause and effect. They say:—"It so happens that during the year 1800, '1, '2, '3, '4, being the five years that immediately preceded those whose statistics I have just quoted, the annual deliveries averaged 500 less than the succeeding ones, being 8,900, yet the mortality was more than double, being 1 in 66." Now, let me ask, why did the deliveries average 500 less, and why did the mortality increase *pari passu* with this lessening? Simply because the overcrowding of the hospitals in the four preceding years, namely, 1796, '97, '98, '99, had engendered the disease, 6,474 patients having been delivered in it in that time, and their admission in 1797 having amounted to 1712. Dr. Evory, whose astute mind and experience pointed out the cause of the increased mortality, consequently limited his admissions, and in 1799, when the disease showed itself, he reduced his deliveries to 1,537, and again in 1800 to 1,337; but it was too late. The disease had found its habitat in the hospital, and continued its havoc for four years.

The indiscreet master who succeeded, Dr. Kelly, would not take the example of his predecessor, but allowed the patients to crowd into hospital, in increased numbers, until the year 1803, when the patients numbered 2,028, and the death-rate reached 1 in 46. Now, as nearly as possible the same thing had occurred in the last three years of Dr. Joseph Clarke's mastership. The deliveries had amounted to 4,990; the highest average up to that time, and in his last year, 1793, they reached 1,757. In that year the death-rate rose from 1 in 163 to 1 in 92, and the disease having found its habitat, the death-rate got to 1 in 77 the next year.

Dr. Evory, however, lowered his deliveries to 1,543 in 1794—in 1795 to 1,503, and in the latter year the death-rate only amounted to 1 in 214. The same thing exactly takes place with Dr. Hopkins. From the year 1806 to 1809 the deliveries increase from 2,220 annually to 2,889. The death-rate rises from 1 in 205, where it stood in 1808, to 1 in 98 in 1810; and although the deliveries are reduced by nearly 400, or to 2,484 in 1813, the death-rate reaches 1 in 40, because the disease has now found its habitat.

A fatal blindness seems, however, to follow our ministrations from the earliest periods of the management of this institution. The diminution of deliveries is attended with its consequences more speedily on this occasion than usual. The death-rate is reduced to 1 in 100. Dr. Labatt's mastership commences in 1815; patients crowd into the hospital again; the admissions reach 3,276 in 1816, and the death-rate stands at 1 in 182. But despite the relief afforded for a time by the increased accommodation afforded by the addition of the blind house, the fatal result, although delayed, is certain. The next year the

deliveries amount to 3,473, the death-rate rises, of course, to 108. Still the experience of former masters is disregarded, the deliveries are permitted to increase to 3,539, and the death-rate increases to 1 in 63; and although when too late, the deliveries are diminished next year to 3,197, the death-rate rises still higher to 1 in 33, the disease having now found its habitat, and retains it for another year, although the deliveries were reduced by 600, or to 2,458.

By a strange infatuation the deliveries were again allowed to increase to 2,849 in 1821, and to 2,675 in 1822; and though lowered in 1823 to 2,584, the death-rate rose to 1 in 44. In 1824 the deliveries were still further lowered to 2,446; the death-rates were reduced to 1 in 122. In 1825 the deliveries increase to 2,746, the death-rate increases to 1 in 105. The habitat is established, and although the deliveries in 1826 are lessened to 2,440, the death-rate rises to 1 in 30.

In this year I first became acquainted with this fell disease, as a pupil of the Lying-in Hospital, and when I call to mind the deep impression made upon my mind by witnessing the death of eighty women in one year by this disease, and of 191 for the four years that I attended as a pupil, most of whom had entered the institution in youth and health, buoyed with the hope of a speedy restoration to their homes and families, it is not to be wondered at that forty years has failed to efface it.

But, sir, painful as it is, I must persist in the cruel task I have set myself. The habitat was now thoroughly formed. The poison miasm pervaded the hospital thoroughly. Dr. Collins's mastership commenced in 1827, and he adopted every precaution that the resources of our art could devise to combat it. The deliveries, however, were increased by upwards of 100 that year, amounting to 2,550; the mortality was 1 in 77. In the next year (1828) the deliveries were increased to 2,856; the death-rate rose to 1 in 66; and although the deliveries were reduced in 1829 to 2,141, yet the habitat was there, and the death-rate was 1 in 63.

The reduction of deliveries, which Dr. Collins never after this allowed to amount so high as those of the next year, 2,288, told, and with the other means so minutely detailed in Dr. Collins's work, diminished his death-rate to 1 in 190, and it did not rise beyond 1 in 178 for the next three years.

It may be better to dwell briefly at this period of the history of the hospital; as, with the termination of Dr. Collins's mastership, an entirely new phase in its history is developed. The conclusions to be drawn from what has preceded are, that on every occasion when the hospital was crowded, sooner or later metria showed itself. Secondly, when this occurred, its habitat became established, and it continued for a longer or shorter time, notwithstanding every means that could be adopted: and although a reduction in the number of deliveries had an effect in ultimately

improving the healthiness of the hospital, even these measures repeatedly failed in abating the evil immediately, when once the habitat of metria was thoroughly established. Let us now recapitulate the third and fourth propositions set forth in my paper, namely: third, that the generation and absorption of the metria poison or contagion is in a direct proportion to the number of parturient females cohabiting at their parturient period, or who breathe the same atmosphere at the time of their delivery; and the fourth:—that in lying-in hospitals, where large numbers of patients are delivered under the same roof, this disease finds its habitat, appearing and reappearing at uncertain intervals.

If ever propositions were established by multiplied facts, and confirmed by prolonged experience, I submit these proportions have been so by the preceding analysis. But, as the object of this paper is, not merely to obtain a barren victory by the ingenious manipulation of a number of figures, but to carry conviction to the minds of the profession, and through them, to the public, let us reason a little more closely upon the laws which appear to be derived from a study of the history of the metria hauntings in the hospital.—Firstly, that markedly as these attacks occur in direct proportion to the number of parturient females cohabiting at their parturient period, yet the contagion is cumulative, or growing in its contagious power, requiring a longer or shorter time to arrive at its full intensity or saturation. This law accounts for the interval of time observed to elapse, before the outbursts of the disease, and after the cause which experience has proved to be the most invariable, if not the only known cause of its development, after it has been generated—I allude to crowding.

Again, although it is true that the spread of the disease is in proportion to the number, it is a mistake to suppose that this crowding *requires* a large number of patients to produce it. We have seen, by the diminution of the mortality in the Waterford Cottage Hospital, on changing the number of beds from six to two in the wards, that it was not the presence of a large number, but the crowding of what we might say a comparatively small number, that sustained the mortality at its higher rate. The same fact is proved by the large mortality observed to occur in the London Hospitals, containing thirty and fifty beds, and in the other hospitals enumerated in Dr. Churchill's table.

Now, ingeniously and pertinaciously as my critics have endeavoured to keep this latter law in the shade, and pervert it as an argument against my proposition, neither their facts nor their logic will hold for one moment on this subject. No incompatibility whatever exists between the latter law and my general proposition. I affirm that a zymotic disease is conveyed by the absorption of poisonous contagion. I lay down a general proposition, that the generation of this poison will be in the direct proportion of the number of patients subject to the conditions on which it is generated, and breathing the

atmosphere charged with it. I require that, to lessen or prevent its generation and spread, the number generating and exposed to the spread of the poison should be reduced to a minimum. That minimum might be one, but, should not object to two, at least, until the success had been sufficiently tested. Need I remind my illogical young critics, that the major comprehends the minor; and that if more than one patient inhabiting a ward increases the liability to generating the poison, and the certainty of its spread and extension, that *a priori* the "generation and extension must be in exact proportion to the number of parturients exposed to it, and breathing the same atmosphere." So far, then, from the mortality in the medium-sized or small hospitals, hunted up by my critics, and a fearful array it was, but especially the mortality in the York-road, the Glasgow, the Edinburgh, and Dr. Beatty's hospitals; I say, so far from their mortality making against my proposition, properly understood, they all confirm and strengthen them, because they present the conditions I object to in a more marked degree; possessing all the objections and vices, with none of the advantages of the great lying-in hospitals.

Dr. M'Clintock, by a process of reasoning of his own, based upon selected statistics, arrives at the conclusion, which I cannot at all agree with as what he terms "above suspicion," that 1 death in 134 is the right number to occur in parturition. And, by another process, he increases the Registrar-General's rate, arrived at from data and sources at least equally "above suspicion" as his own. This he does by deducting one-fourth by his own arbitrary dictum from the Registrar-General's number, 171, for England and Wales, which reduces his ideal death-rate to 1 in 129. Dr. M'Clintock will excuse me, therefore, when I tell him that neither he nor the Registrar-General, were he innocent enough to adopt Dr. M'Clintock's calculation, would be justified, if their object be to give an accurate return of the deaths in childbirth, in deducting upon his abstruse *quaterinian calculation*, which throws Professor Hamilton's into the shade, one-fourth or any other proportion for diseases proving fatal in parturition. Without entering into the subject as to whether these diseases may have been caused or called out by parturition, there is one broad fact patent, that the fatality was determined by the complication of the disease with parturition and non-sequitur, that any one of the cases would have proved fatal at all, and certainly not at that particular time, had not parturition been complicated with the disease, or it may be, produced the disease, and caused or accelerated the death. Therefore the Registrar-General's calculation of 197 mortality is strictly correct, and Dr. M'Clintock's deduction of a fourth is entirely untenable.

Dr. M'Clintock attacks my tenth proposition in his own ingenious manner.

He takes my general proposition and applies to it his own *literal*, not my *general* interpretation, when I state that puerperal fever "is not a disease observed to occur in small lying-in hospitals," and this, although I give the small hospital in which it occurs, with the rate of its occurrence, twice over, once in page 302 and again in page 304. He then devotes a whole page of statistics and argument to prove that I denied that puerperal fever *ever* occurs in small hospitals, and eventually winds up by admitting that my true meaning in the tenth proposition, which was as manifest as the sun in daylight to every eye but Dr. M'Clintock's, "*might* logically be inferred from the admission of the author that the poison of zymotic metria might be generated by any parturient female," and that this disease "is to be met with in the hovels of the poor and the chambers of the wealthier classes." Sir, I shall not designate such a line of discussion as I think it deserves. I ought, perhaps, to apologize for occupying so much of your time in refuting it.

But Dr. M'Clintock, descanting on the Waterford Lying-in Hospital, gives "five deaths as occurring in it from puerperal fever, with an average of only 115 annual deliveries." It would have only been justice to Dr. Elliott and the cause of truth, which we are at least ostensibly investigating, to have added that these five deaths from puerperal fever occurred in the Waterford Lying-in Hospital out of 3,469 deliveries, and extended over a period of thirty years, being a mortality of 1 in 681. A mortality that would quite satisfy me if I could bring it about at the Dublin Lying-in Hospital; aye, or one-half of it!

But, sir, I regret I am not done with Dr. M'Clintock and the Waterford Hospital yet. In adducing these statistics from it, he falls into the very same blunder that, you will presently see, he attacked me for, and out of which he succeeded, by my enforced silence, in making such temporary capital. He fails, unintentionally of course, to distinguish accurately the two periods of the hospital, and, in place of giving the mortality by puerperal fever upon the long, or twenty-three year period, with its improved ratio upon the diminished number of beds, which he ought to have done in common justice to Dr. Elliott and the objects of his argument, *like* me he takes the whole period, but *unlike* me he does not give the results upon the improved period; of course his doing this was, as in my case, a mere slip of the pen in transcription. Let us see what the difference amounts to in the correct way of giving these results. We have seen that in Dr. M'Clintock's manner of proving the rate of mortality, the death-rate of the more crowded Waterford Hospital was 1 in 681; but in the correct and fair way of calculating it amounted only to 1 in 1,328. It will be perceived to prevent mistakes, as Dr. Elliott requires, we have put his calculation, not mine, in letters.

So much, sir, for Dr. M'Clintock's purism in his own calculations,

and his vaunted refutation of my tenth proposition, to refute which, he says, it were enough to cite the instance of the Waterford Lying-in Hospital. The Liverpool Lying-in Hospital is the next cited, but as he gives it a mortality of 1 per cent. it tells in favour of my position, although, as I have already said, I do not admit it as to be classed with the cottage hospitals. I only wish we could reduce the mortality of the Dublin Lying-in Hospital to 1 per cent.

The next instance adduced against the establishment of cottage hospitals is the South-Eastern, or Dr. Beatty's Lying-in Hospital. Dr. M'Clintock introduces his notice of it in these words—"I feel great satisfaction in appealing to the statistics of this little hospital, knowing they are worthy of implicit confidence." I confess honestly I cannot join in the great satisfaction he expresses upon this subject, and feel assured, as was the case in the great lying-in hospital, Dr. Beatty must have laboured under difficulties from the construction of his hospital that rendered it impossible to keep his mortality at a lower standard than 1 in 69. At all events, as I elsewhere stated, I admit no comparison between Dr. Beatty's hospital, with nearly 400 deliveries annually under one roof, and the cottage hospitals already alluded to, and to which we may now add the Killarney and South Dublin Union Hospitals; and be it remembered that these only afford an approximate test of the principle of isolation, as in none of them has the principle been carried out to the full extent required.

The last case adduced by Dr. M'Clintock against my tenth proposition is the small Kingstown Lying-in Hospital. Dr. M'Clintock says that "the annual deliveries, not exceeding 90, *it is in every point* of view an *unexceptionable* specimen of a cottage hospital." Judging from its statistics, I should arrive at a very different conclusion, and as these are my only data for forming my opinion, I must say that I pronounce it in every point of view an *exceptional* specimen of a cottage hospital. When Dr. M'Clintock analyses on the strict philosophical principles laid down by Mill, as applicable to exceptions to general rules, the true explanations of the causes that render it exceptional, then I shall attach value to the exceptional case of the Kingstown Hospital, with its total mortality of 1 in 68, or of 1 in 114 from puerperal fever. In the meantime we must class it with the exceptional case of Ballarat to be afterwards alluded to, as adduced by Dr. Stokes, as a mystery, which, although unsolved, may not be insoluble.

Dr. M'Clintock, as well as several other of my critics, has called in question the statistics supplied by the General Registration Offices throughout these kingdoms. I am not here as the apologist of this department. Those indefatigable and distinguished men who direct and control this great department, one of the few scientific branches of the Executive in this country, stand too high by their labours and results to

require me as their apologist. But, sir, I must be permitted to express my dissent from and regret at the flippant and offensive manner in which this great organization of the State have been treated. Sir, I deplore it the more because I am old enough to recollect the difficulties we had in getting this great desideratum—the Census Office—established upon a fixed and permanent basis; the jealousies it excited, and the difficulties there were to bring our legislators to adopt it, and our masses to co-operate with it and submit to it. It behoves thinking men to consider how far they are justified in breaking the faith of the public in so wise and so valuable a department.

For my own part, I totally disagree with all the slighting remarks that have been thrown out upon the statistical labours of, and results supplied by, the Registrar-General's Office. I rely upon the latter as the best information procurable upon the subject of statistics generally, and I have no doubt that, even admitting it to the fullest extent, that certain inaccuracies must creep into their calculations from accident, different views of seeing facts, or even from design, yet that these are counterpoised by their opposites and corrected by their analogues in such a manner as to render the total results as nearly correct as any human machinery, which necessarily employs an immense and varying agency, can accomplish it. I therefore accept the Registrar-General's statistics as, if not the best possible, at least the best possible with our present facilities, and feel myself quite justified in assuming them as the soundest attainable basis in the present discussion. Their very extent should render them unexceptionable to my critics, as they insist so much upon this element of proving statistical truths. I shall therefore say, that with the greatest deference to those of my commentators who have collected statistics of their own, bearing out their views of what should be the mortality of lying-in women, I shall prefer accepting the statistics of the Registrar-General, and his statement of what it actually *is*, to their selected figures, collected and marshalled to prove what it ought to be. This general statement will dispose of a host of those gentlemen's calculations who have favoured us with statistics of their own selection and procurement, but which are entirely at variance with those of the Registrar-General.

It will now be convenient to allude to another law that demands our attention and consideration. It is conveyed in the word habitat, which you will recollect is the subject of my fourth proposition—one which has been the object of nearly as many attacks as my third; so many, indeed, that my belligerent critics must have looked upon it as the Malakoff of my position. They are right in so doing. So do I.

In answer to those gentlemen who are perfectly satisfied with the state of the Dublin Lying in Hospital, as well as those who have so strenuously and unmeasuredly attacked me for attempting to introduce such improve-

ments into its construction as will lessen the mortality within its walls : let me ask you—some of whom have even had the hardihood to assert that it is, quoad its mortality, in a satisfactory and an improving condition—let me ask you, I say, to look at the facts. The institution has had out of the 113 years passed since its foundation, 65 years in which it was so severely attacked as to bring the death-rate, upon the rough principle of calculation given in my paper, from 3 to 2 figures, and thus 113 divided into the following periods show with what results :—In the first 18 years after the establishment of the hospital tested in this way, metria prevailed extensively in 13 ; in the next 20 in 7 years ; in the next 20 in 8 years; in the next 20 in 8 years; in the next 20 in 13 years; and, in the last 15 years, in every year of the 15, or, in the last 35 years in 28 years, leaving only 7 years free from the ravages of the disease, as thus tested. To this length I was prepared to go, on writing my original paper, and to leave the comparison of two or three figures as the test of the existence or non-existence of metria within the walls of the hospital. The violent and multiplied attacks to which this has exposed me, have naturally required my closer and more searching investigation of this subject. The conclusion which this reconsideration has led to, and confirmed, in my mind, is, that in this rough way of testing the existence of metria by the death-rate amounting to two figures in place of three I was quite wrong and under the mark. That it affords no true test, and that I must abandon it in favour of a more accurate and trustworthy comparison. This I shall be obliged to develop and amplify in dealing with Dr. Beatty's paper a little later, and merely allude to this at present to prevent misapprehension.

We have seen that the condition of atmosphere which predisposes to this disease, is, at times, of very slow development, on what we term cumulative, in its properties. When these properties are thoroughly fermented—this phrase is not mine, sir, it has been used by my critics—and right or wrong in its physiology, it describes what I want to convey. Then, there is often an equally remarkable rapidity in the time occupied in infecting its victim. But when it has once shewn itself, then the law of my fourth proposition comes into operation—the law of habitat; that law which fixes its endemic character, by which the atmosphere is changed from its cumulative to its actual poisoned, or saturated, state. What was an approximation to it, during its cumulative period, becomes now an absolute fixed quantity; and so persistent is it, so obstinate and so pervading, that for weeks, months, and years, as I have proved to you by the history of the Lying-in Hospital, every part of the numerous and spacious wards of this institution has been haunted and occupied by this dire disease. That haunting and occupation has continued, until, like other zymotic diseases, it has worn itself out, or until it has been checked by want of victims; the patients not being admitted into the hospital, offering often the only effectual check to its continuance. One reason why the term habitat is especially applicable to

metria is the peculiarly obstinate and pertinacious character of its hauntings, as compared to other zymotic disease. In cholera, measles, whooping cough, erysipelas, and even small-pox, the disease runs a course and terminates in a reasonable time. It may, in most of the epidemic attacks as they are termed, very questionably I think, be traced to its introduction into a house or neighbourhood hitherto free from it, from a house or locality in which it prevailed. It runs its course—precautions are taken—it wears itself out. The same is observed in epidemic prevalences of puerperal fever, where it prevails in districts. In these it runs it course very much as the other zymotics do: but it is not so in hospitals. Here the law I dwell upon, that of habitat, comes into operation, it continues to haunt the now infected hospital for an indefinite period. Why? Because *here* it possesses the conditions ready for its spread when in existence, and for its regeneration and cumulation, if suspended for a time in its ravages. These are the considerations which justify and require that ill-constructed Lying-in Hospitals should be denominated the habitat of puerperal fever. Need we again recapitulate the proofs of this, adduced so often, but especially in treating of my third proposition? where I showed, amongst other instances, the deaths of 191 women in Dr. Collins's first four years' mastership, including some months of Dr. Pentland's, and 170 in my own seven years' mastership.

This consideration of the habitat, so conclusively proved, brings us, by a natural transition, to the more recent phase or change in the history of the Lying-in Hospital of Dublin referred to, and which embraces the last 35 years. In this period the principles of cumulation and habitat may be said to have culminated and become persistent. In place of occasional outbreaks of metria, with years of health, matters have changed.

This increase, or rather persistency of metria, would lead to the conclusion that true metria is changing in its character more and more every day, assuming the habits and laws of an endemic or indigenous disease in our great hospitals, which, instead of showing itself at intervals, as do other epidemic diseases, remains constant, and will continue to do so as long, at least, as those conditions which generate, cumulate, and develop it are permitted to exist.

This is the state of facts with which we now find ourselves face to face. The phase, the habits, the laws of metria are altered. The bygone history of the lying-in hospital is only so far of value as it traces out the progress of matters to this consummation. We have now not an occasional epidemic, but an established disease, having its generating cause, its cumulative cause, and its pabulum within the four walls of the hospital. The hydra is there within your reach. Deal with it as you would with any other "pest-house." Again, sir, this is the language of my critic, not mine. But, sir, do not misunderstand me. I am in this question, at least, a reconstructive, not a destructive reformer. The

disease, bad as it is, is not in a state to require either disendowment or disestablishment. It merely requires that the hospital should be modified to meet our improved knowledge of zymotic disease, and the altered characters of metria in particular. Let the success of Joseph Clarke in trismus, and of Jenner in small-pox, encourage you in your efforts. Be not deterred by opposition. No valuable improvement was ever accomplished without it, and there is no abuse so untenable that it won't have its backers, no fallacy so palpable that it won't have its supporters. Jenner was nearly annihilated by the anti-vaccinators; he survived them, and the anti-vaccinator is now reduced to a unit. Our profession, enlightened and humane as it is, is not easily turned out of its habitual course of thought and action. Besides, the attractions to our revolving in our present path of thought and action in the case of our large hospitals are weighty and cogent. Let us not, however, try this out too long, or add a third case to the two instances that give the death-rate of the Dublin poor management an infamous celebrity, and one handed down in the statistical books of the last half century as a disgrace and blot to our national character. Let the infant parish mortality and the death-rate of the foundling hospital remind us that, not many years since, there were respectable men, and wise heads, aye, and learned doctors, who did not flinch from maintaining and advocating the persistence in a system of mismanagement, the results of which are too disgraceful to dwell upon, and which led to the erasure from our charitable institution of one which was at that time, what the Dublin Lying-in Hospital has been, the pride of this city—an institution which, with proper management like similar institutions elsewhere, might have been rendered a means of combining a charitable ministration, with a powerful engine of civilization and enlightenment throughout this country.

Sir, I now approach a branch of my subject that I would willingly have avoided. Dr. M'Clintock has detected an error in my transcription, and this is at once characteristically denounced as follows:—"Here Dr. Kennedy has committed a serious arithmetical mistake in his data, which consequently falsifies all the conclusions he has laboured to draw from them." Now, when I tell my hearers that this statement is the substitution of thirty years for twenty-three years in transcribing from Dr. Elliott's book, the period for which an average calculation was adduced; that the result was strictly correct in the average as the basis of calculation founded on the twenty-three years' period; but, above all, when I tell them that the error in transcription was itself corrected, in the same page, by giving Dr. Elliott's text *in globo* in which the two periods were fully set forth, and the calculations based upon them detailed by Dr. Elliott himself, I think I am justified in asking whether it was a worthy or justifiable line for an honourable antagonist in argument to take, to put into circulation an assertion that "I had committed

a serious arithmetical mistake in my data, which consequently falsified all my conclusions," knowing that for weeks or months this damnatory misrepresentation must be in circulation before its contradiction should appear. It will be recollected that Dr. Elliott, in his report of the Waterford Hospital, mentioned that after six years, for which his hospital was established, he removed to another building; and he gives the statistics of this hospital separate from the one he removed to. The statistics of the latter, extending over a period of twenty-three years, I, sir, of course, took the longer period for the basis of my calculation; firstly, because it *was* the *longer* period, and secondly, because there were fewer beds in the wards, and it consequently approximated more nearly to my idea of a cottage hospital. In the hurry of transcription I gave it in my last as the result of the thirty years, not the twenty-three years' period. In the table of figures in the next page (304) I give the accurate result on the twenty-three years' calculation, namely, 1 death in 259, and this it is that forms the basis of my calculation. It is therefore perfectly correct, and I repudiate entirely Dr. M'Clintock's figures, based upon a totally different calculation. It is a remarkable fact that by a typographical error a unit is omitted in the same paragraph that Dr. M'Clintock deals with, but he did not deem it necessary to dwell upon this, as the omission told 1,000 against my argument.

Good, however, comes out of evil, as Dr. M'Clintock's delicate exposure of "my serious arithmetical mistake and falsified conclusions" has drawn out a further commentary from Dr. Elliott, who writes to me to this effect:—"But the most useful and instructive mode of viewing these statistics is reached by dividing them into two periods. During the first period of six and a-half years we occupied an hospital in which eight beds were grouped together in one ward, and there during that period the total mortality was 1 in 125½, and the mortality in puerperal fever was 1 in 251. During the recent period of twenty-three and a-half years we have occupied our present hospital, in which eight beds have been divided between two wards, four beds only in each ward, and here we have had a gross mortality, from all causes, of 1 in 309, and a mortality in puerperal fever of 1 in one thousand three hundred and ninety." Dr. Elliott considerately adds, "I use letters instead of figures, that there may be no mistake this time."

If further proof were required of the too palpable conclusion of the rate of mortality being in proportion to the number of patients occupying a given space, as insisted upon in my third proposition, so violently attacked as the Redan of my position, what could more completely resist their assault and turn their attack than the simple, wise, and thoroughly trustworthy statement of Dr. Elliott, that by the mere expedient of reducing his beds from eight in one ward to four in his two wards, he succeeded in reducing his gross mortality from 1 in 125½ to 1 in 309.

This admits of no refutation, and requires no comment beyond what Dr. Elliott states in his pamphlet, page 6, as a significant fact, "that whereas the mortality from puerperal fever was in the first hospital 1 in 251 during a period of six years and six months, the mortality from the same cause has been, in the present one, only 1 in 1,328.

Dr. M'Clintock's best specimen of statistical criticism remains. It is that in which his anxiety to arrive at truth is only equalled by his determination, in his own language, "to show my chain of reasoning fallacious," "inasmuch as a great error, represented by the number 8,688, lies at the very basis of my calculations, and vitiates *all* the conclusions derived from them." "The error is this: I give the total number of births in the Dublin district for the period specified as 52,126, whereas it is only 43,438." Dr. M'Clintock then naively adds—"I discovered this by the merest accident in the library up stairs when looking over the reports of the Registrar-General for Ireland;" and then, feeling that he has gone to the very verge of the line of veracity in his insinuation, adroitly turns the subject with the most inimitable simplicity by the words—"Let me now return from this digression." Now, let me ask, if the object of these remarks was fair and candid criticism, why did not Dr. M'Clintock show to his hearers how this mare's nest of 8,688 of an error vitiates all or any of the conclusions derived from this calculation? Simply because he could not, as the calculation was not in the slightest degree affected, nor the results in the most remote degree influenced by the mare's nest he had discovered. Dr. M'Clintock knew when he penned this that I was discussing an average from these figures, and that whether this average was produced from forty or fifty thousand did not signify. Hence, then, the graphic description of the "accident in the library when looking over the reports." I had nearly forgotten to add the interesting detail, following the example of Mrs. Quickly and her seacole fire, in order to insure precision and extreme accuracy. Dr. M'Clintock informs us that it was "upstairs;" and hence, also, the sudden and adroit, but, alas! now that the day of *reckoning* has come, the simple suggestion, "Let us return from this digression," will not avail in relieving him from his dilemma when I tell him, as I do, that the duty remained to him to prove in what manner the 8,688 "had vitiated all my conclusions." Sir, that was a duty he could not execute, because it was impossible for him, by any amount of ingenuity, to prove as a fact what was not a fact, and what he put forward as a fact, although the means of testing its falsity were before him "upstairs in the library," just as patent as they are before you and me at this moment.

The fact is, the 8,688 error was totally unimportant in transcription, and whether it was 8 or 8,000 did not affect the result one iota. You will perceive by this return, kindly furnished me by the Registrar-

General for Ireland, and given beneath, that the total number of births are given in six consecutive lines of figures thus:—

	1864	1865	1866	1867	1868	Average number 1869, 5·8
The first five take in the year, the 6th gives the average.						
Now, the same table gives, lower down, the deaths from puerperal fever and metria in consecutive years also, and with the average the last column, thus:—						
Deaths from puerperal fever, - -	42	32	66	37	32	42
,, in childbirth, - - -	18	35	27	38	46	33

In transcribing the above numbers into a column for totting, the sixth line of figures was added in both cases from the words "average numbers," written in a small hand, causing them to escape my notice. Thus the average of births was given as an additional year, as well as the average of deaths, leaving the resulting average, which was the object of the calculation, identically the same as if both had been omitted, and the five years only given. This, then, is the great error discovered upstairs in the library, that "renders my whole chain of reasoning fallacious, and vitiates all my conclusions."

But, sir, as this great error has been mooted in all the medical coteries of Dublin, and will fly over medical Europe on the wings of the press, I have deemed it due to myself and to the important subject into which I have thrown my advocacy, to place these *great errors* of transcription, so much dwelt upon, in their right light. I have, therefore, required the certificate of Mr. Francis Low, whose reputation as a notary public and statistician rank so deservedly high as to their true nature and their bearing upon the subject under discussion. Here is his certificate:—

"At the request of Dr. Kennedy I have examined his computation of the rate of mortality from puerperal fever and childbirth which prevailed in the Dublin registration district during the five years from 1864 to 1868 inclusive, as given in his paper on "Zymotic Diseases," and deduced from the Registrar-General's return, and find that, notwithstanding an error in the transcription of the figures, *the result is correct*, as will be seen by the following explanation:—

"Dr. Kennedy, in summing up the total number of births, as also of deaths (resulting from the causes stated), for the five years alluded to, included, in error, a sixth quantity, viz., the average for the five years, which had been placed in a sixth column over against the other five, giving the births and deaths referred to. The including of this sixth quantity, though inadvertent, *plainly cannot affect the result*, because *it* is the mean (as stated) of the other five, and therefore the mean of the six must be identical with that of the five, and the mean number of

deliveries, divided by the mean number of deaths, constitutes the rate of mortality sought.

"Dr. Kennedy also quotes correctly Dr. Elliott's report of the rate of mortality (1 in 295) prevailing in the hospital in Waterford intended to be referred to, viz., that occupied as such from October, 1844, the only errors being the statements that Dr. Elliott's report regarding the mortality in this hospital extended from 1838 to 1868, instead of from 1844 to 1867, and that the mortality from puerperal fever was 1 in 328 instead of 1 in 1,328, which latter rate manifestly strengthens Dr. Kennedy's position.

"Francis Low, Not. Pub.

"Dublin, 26*th June*, 1869."

Sir, in my ignorance I had imagined that in a large mass of calculations such as the preparation of my paper necessarily involved, errors in transcription and typography, and even oversights in calculation, might be expected to creep in. I further imagined that in a discussion for the benefit of science and humanity, such as this purports to be, an honourable seeker of truth would have shrunk from making use of such accidental oversights as weapons with which to damage his opponent. However I have lived to learn. But, sir, I am too old to learn much. Therefore I cannot undertake to benefit to the full extent intended by the lessons I have had in arithmetic.

Be that as it may, I may say this much, that should I be required to advise a course of education to qualify a man for a similar encounter to that in which I have been engaged, I should be disposed to take a lesson from the schooling I have had, and advise his being educated upon the excellent system so admirably laid down by Mrs. Malaprop, and which you, no doubt, recollect she so judiciously insists upon in allusion to her female pupil. "I should not allow her to meddle with logic, algebra, simony, fluxions, paradoxes, or such inflammatory branches of learning; neither should I let her trouble herself with mathematical, astronomical, or diabolical instruments. But she should be taught a supercillious knowledge of accounts; sent to school to learn a little ingenuity and artifice, and as she grew up she should be instructed in geometry, that she might know something of contagious countries, but, above all, she must be a true master of orthodoxy, in order that she may not pronounce her words so shamefully as people usually do, and that people may reprehend the meaning of what she was saying."

The pleasing duty now devolves upon me of commenting on my friend Dr. Beatty's paper, which I look upon as one of the most valuable read to the Society throughout this debate. He took up one idea, he never lost sight of it throughout, and he exhausted it so completely that every commentator who has dealt with his subject has been feeble in

comparison to him upon it; I allude to the identity of puerperal fever and erysipelas. His arguments, cases, and reasoning leave nothing to be said upon this subject, if indeed further evidence was required to confirm the views propounded by Dr. Sidey thirty years ago, and sustained by almost every hospital physician who had possessed an opportunity of observing upon those diseases since. Dr. Beatty is equally strong upon the contagious nature of this, according to him, common disease, erysipelas and puerperal fever, and his observations upon this branch of his subject merit our deepest attention. In fact, Dr. Beatty's support of my views is conclusive, and merits my warmest acknowledgement. Nevertheless it was necessary that he should also attack me; but I am puzzled to know why he, of all my commentators, should have been the person to read me and this Society a lecture upon the waste of time occupied in vain attempts to discover the nature, origin, and best means of preventing metria, and to prohibit speculation on the origin and common nature of zymotic poisons, in the very same breath that he advocates and insists upon the identity of two of these poisons, erysipelas and puerperal fever. Dr. Beatty asks this Society, in his own innocent manner, if any one is a bit the wiser after reading a passage which illustrates common poison on the principles of isomerism, as advocated by Dumas, for ignorance of which and its application to the subject in hand, any enlightened physician might justly have been condemned.

Dr. Beatty brings a charge against this Society of "*one-sidedness* as applicable to their recent proceedings, their speeches, and their arguments in contravention of my paper." This is his language, not mine. Sir, I should have scorned to bring such a charge against our common profession, whatever insinuations Dr. Beatty, or even the all-powerful press of this country, may make to the contrary. Well may his congeners say—had an enemy done this thing, but that it should have been done by our own familiar friend with whom we had taken sweet counsel together; and he adds, that not one single voice has been raised in favour of my paper. Why, sir, Dr. Beatty himself is a living instance to the contrary; his facts, his arguments, and his reasoning go most strongly in favour of my positions. I could not have wished for a better advocate, or more conclusive evidence than he adduces for my views—and he possesses this great merit, that he is perfectly unbiassed towards me. I can, therefore, only explain the hostile line he has taken by his labouring under this strange hallucination, that all the speakers on my paper are "one-sided," and conclude that his national tendencies so predominate that he must strike a blow on the one side even whilst his arguments go to support the other, on what he so naively designated this "one-sided discussion."

Dr. Beatty sets out by another rather sly cut at his friends in telling them that *their statistics have so well answered the purpose for which they were used*, that he will not weary the Society by any allusion to that

dry subject, further than by giving a table of his father, the late Dr. John Beatty, and his own statistics in private practice. Their total combined deliveries were 7,680; deaths, 30; being a mortality of 1 in 256. I believe this death-rate to approximate much more nearly to the true general rate in private practice than the examples given in Dr. M'Clintock's or Dr. Churchill's tables.

I, therefore, accept them as far as they go as confirmatory of my views and calculations.

Dr. Beatty then passes to the support of my views upon a common poison, and succeeds in establishing the identity of two at least, indeed I might say three, ranking pyemia as one. This is a first step towards the accomplishment of what may eventuate as a more general recognition of this principle. He modifies this statement when he says we know that zymotic diseases, but more especially measles and small-pox, are not convertable the one into the other. But, it will be in the recollection of the Society, that Dr. Stokes met this difficulty by admitting the occurrence of *mule diseases;* therefore, on these nicer points, we may safely leave Dr. Beatty and Dr. Stokes to fight it out.

I might here take an exception to the pathology of Dr. Sidey and Dr. Beatty, and state that the great variety of tissues engaged, and the not infrequent absence of peritonitis, and other serous inflammations, in metria, do not quite justify the conclusions that "puerperal fever is merely an erysipelatous inflammation of the peritoneal covering of the pelvic region, or other serous membrane, extending, as the disease advances, over the abdominal cavity, and the body of the uterus accompanied by true erysipelatous fever." Dr. Beatty says that when erysipelas is epidemic, metria is sure to appear, and that when erysipelas subsides so will metria, "if *separation* for a time be enforced, and the disease not perpetuated by contagion."

This latter admission is valuable, and the Society should not allow it to escape their notice, that on Dr. Beatty's authority, the condition upon which this common disease subsides is separation, which condition not being complied with, it does not subside. Could this gentleman have used a stronger argument in favour of isolation and the establishment of cottage hospitals? I think not.

Dr. Beatty next attacks my statement, that metria approaches more nearly to a constant quantity in great lying-in hospitals; and asks me why it is not always present in such institutions? and why it is more intense when the greatest numbers are collected together? which he answers is notoriously not the case.

Now, sir, is it really necessary for me, with the statistics of all the great hospitals of Europe before you, and with those of your own hospital at your door, to go back and prove to this Society what Dr. M'Clintock has denominated, "*the great excess of metria cases in hospital.*" If language

expresses facts, or facts are capable of expression by language, what language could more accurately express this state of facts than that metria approaches more nearly to a constant quantity in a great hospital; but indeed the tenor of Dr. Beatty's own paper goes to prove this, although he explains the excess of metria on different grounds.

I may here mention, in addition to the dread array furnished in my former paper, that in the Maternity at Turin, where 700 or 800 women are annually delivered, the mortality for the last 10 years amounted, according to Professor Faye, to 1 in 25, or 4 per cent. in the students' department. In the department for midwives it is said to be still greater, which fact, together with the more recent experience of the Vienna hospital, would appear to lead to the conclusion that too much stress may have been laid upon the extension of contagion through dissections. However, as this is a step in the right direction, we shall not dwell upon it.

In the lying-in hospital in Dresden the average mortality amounts to from 2 to 3 per cent.

The lying-in institution in Copenhagen, according to Dr. Stadfeldt's report, gives 1,237 deliveries in the year, with 41 deaths, 1 in 30, or 3·3 per cent. This approaches very near to the mortality in the Dublin Lying-in Hospital as it does in the number of deliveries. This report, however, is stated to be, on the whole, more unfavourable than that of the preceding year.—*Med. Chir. Rev.*, Jan. 39? T. 26?

On the other subject, the mortality in proportion to the numbers, Dr. Beatty offers a very simple, but to me inconclusive denial, that "it is notoriously not the case." The Society might not, perhaps, be quite satisfied were I to answer Dr. Beatty in his own language, and simply assert that it notoriously is the case, as I have proved by the statistics of the lying-in hospital, and it notoriously has been the case since its foundation. On no occasion in which the hospital was crowded was not the crowding succeeded by metria, and the metria was not banished until the numbers were lessened, and separation, as insisted upon by Dr. Beatty, carried out. I explained this upon a principle, which must attract more attention than it has hitherto done, in the investigation of zymotic disease; namely, the principle of cumulation. The duration and development of zymotic disease generally, will acquire much elucidation by the study of the habits of metria, and the application of the principle of cumulation in our great lying-in hospital. Dr. Beatty asks "why have there been long intervals of years, sometimes when the largest number of women have been confined during which no metria has occurred?" In the first place, I deny the long intervals of years. The tables of the Dublin Lying-in Hospital prove directly the reverse. Let us test this by Dr. Collins's death-rate, calculated from the one solitary instance of four years of freedom

from the disease, which stood at 1 in 184. Now let us see how many years of the 111 that the hospital has been open for, have ranged above, and how many below this death-rate. Out of the 111 years, 104 have ranged above it; therefore, we might insist upon 104 years out of the 111 having exhibited metria; but, as this would include some years that approximated too closely to the limit, we shall strike off thirty from Collins's average mortality, and calculate that metria was present in the hospital every year that the mortality arose to within thirty of his average death-rate for his four successful years—a calculation that can not be objected to. Now what is the result on this calculation? that out of 111 years of the hospital's existence it has only been free from metria for eighteen years, and has been haunted by it ninety-three years, or more than five-sixths of the time that has elapsed since its opening. Again, let us test the assertion of the long intervals of years without metria, when the largest number of women, were as Dr. Beatty says, confined; an assertion in which he by no means stands alone, as Dr. M'Clintock, Dr. Atthill, and several other learned doctors too numerous to mention, make the same assertion. If we abstract Dr. Collins's exceptional period of four years' freedom from metria, which, providentially affords us a sufficient duration of healthiness to base sound sanitary calculations upon, in analysing its history, for no period, throughout the existence of the institution but one has it been three years free from metria. That was from 1781 to 1782 inclusive. Only twice has it had two years' freedom, namely, 1807 and 1808; and, in the fourteen years of the eighteen for which it was free, it was only for one year at a time that the immunity existed. No long time for the cumulative process to develop its poisoning principle.

So much for the assertion of the long intervals of years in which no metria has occurred, and for Dr. M'Clintock's joke of the long period of gestation.

Let us now turn to the appendix, cast our eye over the tables, and test the effect of crowding by showing the relation in which increased numbers have stood to mortality, from the foundation of the hospital to the present day. I regret delaying you with this, but it is imperative.

Let my readers also examine the table of curves, which indicates this relation at a glance, for which I am indebted to my friend Dr. Wrigley Grimshaw, our authority upon this admirable plan of illustration. If this, with the detailed enumeration set forth in the appendix, fails to satisfy the most sceptical of my opponents of the relation between crowding and mortality, and of the Dublin Lying-in Hospital having been for 97 out of 111 years of its establishment the habitat of metria, as well as of its being now a persistent endemic in that institution, it only remains to decide whether the failure results from want of proof, or from wilful blindness and their being incorrigible.

I have, in the appendix, gone into the details of each year of the last or altered period of the hospital, since the disease became truly endemic, to show that even in this period a further increase was added on each occasion to its permanent high rate of mortality by increasing the number of women delivered under the same roof, in the same manner exactly as was the case when the disease assumed more of the epidemic character in its attacks; I mean in those periods of the history of the institution when occasional healthy years cropped up, with alternating outbreaks of metria more or less severe.

This brings our annual analysis of the hospital to its conclusion, and confirms with a confirmation, "strong as holy writ," our two positions, the fourth and fifth, that have been so much attacked, and which we submit are as now placed beyond the possibility of cavil.

Dr. Stokes states, in his observations upon my paper, that there are two elements in the production of a disease from poison, such as metria, receptivity, and poison. Now, with great deference to my learned friend, if he will read my propositions correctly, he will find there are three. The first is the being a parturient female. This may be defined as the condition-precedent. The second is the existence of circumstances favourable to its imbibition; and the last is the existence of the poison.

Dr. Stokes overlooked the second altogether, and disregarded his logic by the substitution of a middle term different from mine when he put such language into my mouth as—"That a natural process, such as parturition, should be *of necessity* coupled with a tendency to develop a fatal poison."

My language, my ideas communicated, and my logic, are exactly the reverse of this. My proposition stating—"That this poison *may* be generated *by any* parturient female, and *where* the *circumstances* are *favourable* to its imbibition it *may* be absorbed into the system of the generator, or that of any other parturient female exposed to its influence."

From this it is patent that my intention was not to convey such an extravagant or erroneous idea, as that a natural process, "such as parturition, should *be of necessity* coupled with a tendency to develop a specific and fatal poison," which, no doubt, would have been, as Dr. Stokes characterizes this creature of his own fancy, "a startling proposition." But to convey what *is* conveyed as plainly as language can convey any idea, *ipsissimis verbis*—"That this poison is not of necessity generated by every, but *may* be generated by *any* parturient female *where* the circumstances are *favourable* to its *imbibition*, and absorbed into the system of the generator, or that of any other parturient female exposed to its influence."

I am not, then, placed in the position that Dr. Stokes had designed to land me in, of proving that the "natural process that secured the

existence of man upon the earth is necessarily coupled with a tendency to develop a specific and fatal poison."

Dr. Stokes next lays down a very sound rule in philosophizing that we should, where there are two methods of explaining a certain phenomenon, accept the most probable. Now, let us test his own views upon this principle. He says, I myself have not the slightest doubt that "contagion is less the cause of the spreading of puerperal fever in *hospitals* than epidemic influences. What these epidemic influences are no man can say," &c. Again, one of the most remarkable instances with which I am acquainted in favour of the epidemic influences in producing puerperal fever as distinguished from contagion in lying-in hospitals "is the great fact that women are simultaneously affected by puerperal fever in the hospitals where they have been entirely separated, and where even the attendants have not been the same."

Now, let us apply Dr. Stokes's test of probability to this position. Is it possible, let me ask him, to find the state of things he describes in the lying-in hospital without a case of puerperal fever in the district outside of it, a state confirmed by my own experience as well as that of most other masters of the hospital; and yet to persevere in ascribing this disease to epidemic, not endemic, and contagious influences as the more *probable cause?*

Is it more probable that a disease that has haunted the Lying-in Hospital of Dublin, as I have proved by its statistics for 97 out of the 111 years it has been opened, should be more due to endemic and contagious causes, or to epidemic causes, when the districts surrounding the hospital, taking all the years it has prevailed epidemically in Dublin would not amount to a tithe of those in which the hospital has been scourged with it? Is it more probable that a disease existing, and repeatedly existing, *in* the hospital and *not* appearing *outside* should be due to causes operating within the hospital, and not to causes operating without?

But Dr. Stokes begs the question in his proposition. He says the women are simultaneously affected. This is not my experience of even what we term an outburst of puerperal fever. On the contrary, patients are consecutively attacked. Unlike the occurrence of influenza—the best specimen of epidemic disease—which often spreads to hundreds of cases in the course of one day or night, and depends, as we often observe, upon sudden atmospheric changes, metria begins usually by a dropping case, then in a few days another, then the plot thickens as the atmosphere becomes pervaded with the poison, and fresh victims are in the state of receptivity. Again, the influenza disappears or dies out with the alteration of the physical or atmospheric causes that produced it. Not so metria in hospitals, as we have had too painful evidence in what has preceded. Is it, therefore, *more probable* that with this state of consecutive

development of the disease, it should not be more ascribable to endemic and contagious than to epidemic causes?

But Dr. Stokes again begs the question. He says, "Where they have been entirely separated in hospitals the disease has spread." But have they been entirely separated? This is the very point I have been devoting all my energies to accomplish. This is the very question at issue—Whether they are to be entirely separated?—and this is the very view of the question that I regret my friend Dr. Stokes has thrown in the weight of his great influence to prevent. If he will join me in this one point—namely, the entirely separating the patients, I shall hail him as an advocate of the cause I have so much at heart. But what is the fact? It is impossible, and I know it by painful experience, "to entirely separate" a number of people living under the same roof, and who must necessarily breathe the same atmosphere. I have tried it, and I might just as well, to use a homely adage, have attempted to stop the tide with a pitchfork.

But Dr. Stokes alludes to the selection made in patients being attacked or contaminated in different parts of the same building, and looks upon this as incompatible with contagion or endemic influence. Why, it is the very essence of its nature that contagion, *that* the pestilence that walketh in darkness should charge an inclosed atmosphere and hang about or haunt it, moving from room to room with the currents that invade the building, resting for a time in one from stagnation, and then moving away to another. And here Dr. Stokes's receptivity comes into play in explaining the selection of its insidious attack. The circumstances are favourable to its imbibition, and the victim is struck. What can be more probable than all this? Contrast with it the probability of an epidemic metria, which *does not exist outside*, permeating the hospital, selecting one victim after another, within its walls, say even in distant chambers, and never going beyond the hospital, the disease dying out after a time on the law which regulates these strange variations, returning in the same manner; and this often occurring, again and again, as I have known it to be the case, before selecting some cases for attack in the district. These cases frequently traced, as I have done, to the patients of the same practitioner, who was at the moment attending the infected hospital, and thus dying out in the district only to crop up in the hospital again at a short interval. And then let us repeat the question, to which cause, epidemic or contagious and endemic influences, are we to refer hospital metria as most probable? Surely there can be no hesitation, to use Dr. Stokes's own language, that of these two methods of explaining the phenomenon, metria, we should accept that which is most probable, and which, with much deference to him, I submit I have thus, on his own test, established to depend upon endemic and contagious, not epidemic, influences.

But, sir, before leaving the subject of epidemic, the derivation of the word implies that it is a generally prevalent and atmospheric pervading influence, operating physically upon man, animals, and plants. It may be by alteration in the physical properties of the atmosphere simply, as sudden changes from heat to cold, from drought to moisture, or the reverse; or from changes either thus indirectly or directly produced in its electricity. Under this class of cases may be mentioned as epidemic, influenza, rheumatism, facial paralysis, dropsies, produced by checking the insensible perspiration, &c. It may be from the loading of the atmosphere with a noxious or poisonous exhalation of a vegetable or earthy nature, such as ague or those fevers of tropical climates, pervading certain localities, and from which our soldiers suffer on too near approach to the coasts. Or it may be from the atmosphere being loaded by animal poisons or exhalations, such as we observe from defective sewage in cities, when typhoid fever, diptharite, and typhus prevail. Or, lastly, it may be a disease requiring contact or approximation of the diseased with the person to be affected, and ascribable to the direct communication of a poison from the one to the other. Whooping-cough, the exanthemata mumps, furnish us with such examples.

But why do we dwell upon these truisms? Because there is a great scientific distinction and a great practical lesson to be learned from their recapitulation and study. The first group described should be classed as quite distinct from the others, as all pervading, and not necessarily depending for its production upon the contamination of a poison. It is non-preventable, and in its attacks all are susceptible to its influence. Nevertheless, some of the diseases of this class, as influenza, are capable of transmission subsequently by a poisonous influence generated in the person attacked.

The second group described, that produced by noxious vegetable exhalations or poisons, is in simple ague the form in which we are most familiar with it: a disease that retains the germ of the poison for a long time in the system, lying latent for weeks, months, or longer, and seizing the patient at intervals, apparently capriciously; although the seizures generally recur on exposure to an ague or swampy district. This, however, may be looked upon as a fresh poisoning in a susceptible patient. This class of case is incapable, generally, of communicating its poison to another. In the class of diseases ascribable to foul animal poison, typhoid fever is conspicuous, and this disease is now well known not to be communicable by contagion. Not so typhus; it is purely a contagious disease, generated and developed in crowded and animal-tainted atmospheres, especially in those predisposed to it by debilitating habits. Like erysipelas and metria, it is a disease which may be either communicated to the patient, extending to her from another contaminated or poisoned patient, directly or indirectly; or it may be, as we have stated in our

first paper on metria, generated in the patient herself when exposed to the influences that generate and develop it.

Now, the reasoning resulting from this bird's-eye view of these zymotic diseases amounts to this, that we have had hitherto very vague and indistinct notions of epidemic, endemic, and contagious diseases as to their laws and habits of generation, development, and extension or spread. Our want of precision upon them has led to most disastrous results, by causing us to look upon diseases as epidemic, and consequently beyond our control and prevention, when they were due to endemic causes that were easily remediable, or to contagion that had only to be prevented by strict attention to separation of the diseased from the healthy. The light that the investigation of the habits of cholera in its generation in crowded caravans amid the fetid Indian rivers, those of yellow fever in New Orleans, and those of plague at the sepulchre and haunts of the pilgrims in Egypt, has thrown upon the so-called epidemics, has gone a step towards correcting our views on this subject of epidemic and contagious influences—both influences preventable in their character, and which are the root of our enormous mortality and scourging by diseases which belong to their groups. What says an able writer on these zymotic diseases, in a recent number of that excellent popular journal the *Echo*, showing us that the public are becoming alive to these distinctions, although we may shut our eyes to them. In commenting upon Dr. Farr's admirable *résumé* of the causes of death in 1867, he writes:—"The origin of these diseases may be traced to the propagation of certain molecular atoms, not as yet scientifically classed or understood"—(This, I may say, is scarcely a matter of astonishment, when in attempting to investigate this subject by bringing Dumas' views upon isomerism to bear upon them, the bare attempt was ridiculed as beyond our comprehension by a worthy and excellent friend of mine, Dr. Beatty, who, at no distant period, sir, sat in your chair, and at a more recent one graced the chair of the President of this College, but who evidently thinks otherwise.) To return to the *Echo*—"It may be traced," he says, following Dr. Farr, "to certain living molecular atoms, not as yet sufficiently classed or understood, which, entering into man's body, transmute his tissues into their own substance, so that he can no longer live his own life. The process is analogous to fermentation, and each zymotic atom is the little leaven that leavens the whole lump. Hence"—(he continues, in language plain enough, I am sure, but I fear not plain enough to convince an unwilling audience)—"Hence the danger of overcrowding, for the aggragation of numbers within narrow limits facilitates the transmission of the seeds of the disease."

Dr. Stokes admits that if I establish my position of the relation between the number of women delivered under one roof and the mortality, that then he must admit that zymotic disease acquires a great impulse

from crowding, and on the same grounds also, that he must believe with me that the parturient woman has in herself a power of generating a poison which infects herself, and which produces a similar process by contagion in her neighbours. Now, I submit I have proved this relation to a demonstration by the enumeration of the 111 years' statistics of the lying-in hospital, and disproved its converse, which was unnecessary, as the proof of the affirmative was sufficient, by reference to the most authentic and reliable sources of information available, notwithstanding the few instances adduced to the contrary by certain of my commentators. But least he require further evidence, I may refer him to Dr. Farr's last report just published, in which he will find in contrast to the grim death-rate, 1 in 31 of the Dublin Lying-in Hospital for the last fifteen years, Dr. Farr gives as the average of the last twenty-one years in England and Wales, the rate of mortality in child-birth as 1 in 200. In the healthier parts of England there are 43 deaths in 10,000 deliveries, or 1 in 232; in Wales the deaths amount to 61 in 10,000, or 1 in 164, which difference is ascribed to what may be called the civilization of the districts, the three last years in England and Wales being in 223. Thus although the general average of twenty years has improved throughout England and Wales by 1 in 23, it has in the last twenty years nearly doubled in the lying-in hospital.

Sir, although Dr. Stokes arrives at the conclusion that self-poisoning in puerperal fever is not proven, there are those who have thought deeply and written well upon this subject who are of a totally different opinion. Dr. Graily Hewitt, for instance, who has written more to the point in 21 pages of pamphlet upon the disease than any writer I am acquainted with, says:—"It is impossible to escape the conclusion that it consists in nothing more or less than an introduction into the general circulating fluid of a poisonous material of animal origin—that it is a form of pyemia, for the production of which the minutest portion of the morbific agent may prove sufficient." But, sir, he goes further, and agrees with me in the self-poisoning. He says:—"That puerperal fever may occur in a well-marked form, and apart from such introduction from without of morbid material, is undoubted, but in such cases the explanation is virtually the same—the secretions from the surface of the uterus may become fetid, and may also be absorbed, in which case we have the idiopathic disease."

But Dr. Hewitt is not the only physician who has arrived at these conclusions, had we time to dwell upon them.

Dr. Stage, of Copenhagen, after treating of catarrhal affections, and uterine lesions in particular, in connexion with sporadic metria, in which he quotes Winckel, adds:—"The explanation of almost all these cases is, therefore, this, that the individual herself, in some mode or other, develops putrid matters, which are absorbed, and produce an infection—the so-called self-infection, in contrast to an infection from without."

Sir, there are some facts so conclusive to our mind that they do not require proof, and some equally convincing although they do not admit of proof. Let the learned professor but look to the sun descending behind the horizon this evening, and then prove to me that he will ever see it on the morrow; but does he doubt it? Sir, although I draw his recollection to this fact, I am not prepared to say, nor is he perhaps prepared to deny, that the principle of self-poisoning does not admit of proof conclusive to the mind of any reasoning man. Can he deny it in empyema? can he deny it in syphilis? Then why deny it in the case of metria, which, to my mind, is the most conclusive of all? Let us test it by the simplest rule of evidence, as glanced at by himself. There is one axiom upon which all jurists agree, an axiom which rules the decisions of our judges in dealing with our lives and property, as laid down by Lord Mansfield:—"As mathematical or absolute certainty is seldom to be attained in human affairs, reason and public utility require that judges and all mankind, in forming their opinion of the truth of facts, should be regulated by the superior number of probabilities, on the one side or the other, whether the amount of probabilities be expressed in words or arguments, by figures or numbers."

Again, Philips says:—"It is of the utmost importance to bear in mind that when a number of independent circumstances point to the same conclusion, the probability of the justness of that conclusion is not the sum of the simple probabilities of those circumstances, but is the compound result of them."

Now, testing the comparison of proof between the probability of poisoning by atmospheric or epidemic influence, with self-poisoning by an inherent generating power in the individual attacked sporadically by metria, when no other case in the district is attacked, who can for one moment hesitate in concluding, after what has preceded on the subject, that, with Mansfield, the superior number of probabilities—and, with Philips, the compound result of them—go to confirm the self-poisoning; whilst, with Butterworth, the force and effect of the circumstantial evidence depending upon the incompatibility of the explanation by atmospheric influence, or in fact upon any other supposition, reduces anything else to an approximation to an *argumentum ad absurdum.*

Sir, Dr. Stokes, as others of my commentators have done, has availed himself of the opportunity afforded by this discussion of pronouncing a censure upon the Registrar-General's Department. He says, "The reliability of the returns of deaths to the Registrar, as far as puerperal fever is concerned, is worth very little." "I can state that, from long experience. Now, I think it hardly fair to compare these statistics with those of a regular hospital." Such a condemnation as this from a physician of Dr. Stokes' weight and eminence, is no trifling matter, and yet Farr, Wilde, Donnelly, and Burke, are

not men likely to have lent themselves to the compilation or circulation of unreliable returns, or statistics that would be worth very little. I am quite aware that in all statistics capable of collection errors must creep in; nay more, that the statistics collected at present are capable of much improvement. Dr. Farr himself uses nearly as strong language on this subject as Dr. Stokes. I am also aware that in determining the exact class of case which should be denominated puerperal fever, a difficulty often arose, but this difficulty applied equally in the hospital returns as those out of hospital. Indeed Dr. Stokes seems to have admitted this when he adds, "It also seems to me that we are very deficient in accurate statistics as to the mortality from puerperal fever in hospitals and private practice." If such be the case I look upon Dr. Stokes' objection to the comparison of the statistics in and out of hospital as untenable, on his own line of reasoning; as the same elements of what he terms unreliablity, and worthlessness extend to both. In adducing this answer to Dr. Stokes' objection, whilst I freely admit that a difficulty did exist in statistics classed under the head of puerperal fever, which applied equally to all statistics, a difficulty which is much remedied by adopting the general nomer of metria, I object by my silence to appear to approve of the language applied to the Registrar-General, and to the wholesale attacks made upon a valuable department of the state, which, as scientific men interested in the acquisition of sound knowledge, to constitute the basis of our reasoning in diseases, it is so much our object to sustain, and our duty to respect.

We have now a matter to deal with in Dr. Stokes' statement, the consideration of which no doubt will go some length in explaining the line of argument he has adopted in this discussion. He utters the following language which, with the weight of his authority would prove conclusive, were it not that he himself so markedly disclaimed all knowledge of obstetrical practice in his opening observations:—"Now," he says, "If the mortality in private practice with all these favourable circumstances, be as great or greater, which I believe it to be, than in the lying-in hospitals, what becomes of the argument that the fatality is in proportion to the number congregated under one roof?" Now, I cannot for the life of me dismiss the idea that Dr. Stokes has by some unusual to him obscurity of thought confounded the words greater in degree and greater in quantity, or absolutely greater, and what leads to this conclusion is that in the preceding sentence he says, "I have been repeatedly called in consultation to cases of puerperal fever in the better class of life, and I am sorry to say the result is, that in these forty years I have never seen one case of puerperal fever in private practice that was not fatal; not one."

This general sentence of death, pronounced by Dr. Stokes upon all cases of puerperal fever, evidently astounded the Society, and called

for a pointed inquiry from Dr. Murney. I can, therefore, quite understand why Dr. Stokes should have *felt himself* justified in pronouncing a greater mortality in degree, as all *his* cases in private practice were fatal; and greater than this there could not be, and possibly even as *far as his own experience went*, a greater mortality in quantity, as he does not tell us whether he even attended cases of it in hospital. But the general tenor of his conclusions would lead to a contrary conclusion. Therefore it is quite possible that he might have been justified in drawing the same conclusions from his own experience in quantity also. But it remains for the Society to be satisfied or otherwise with this conclusion, more especially when they recollect the annual statistics of mortality for 111 years, the detail of which they have had to submit to as administered at my hands, and Dr. M'Clintock's honest admission of the great mortality in lying-in hospitals.

I therefore submit that if Dr. Stokes has no stronger grounds for refusing his assent to the greater proportional mortality in lying-in hospitals from puerperal fever than the result of his own practice with his reasoning upon it, that I am justified in pronouncing that he has arrived at an impotent conclusion.

I shall not, however, be so ungenerous as to withhold from the Society a fact with which, as our object should be more to arrive at truth than victory, it should be acquainted—namely, that Dr. Stokes does not stand alone in his convictions of the mortality of puerperal fever generally. Sir Charles Mansfield Clarke, whose experience as an obstetric physician was very great, has uttered the same opinion.

Dr. Joseph Clarke writes in March, 1829 :—"In looking over the *Medico-Chirurgical Review* for January, 1829, my attention has been excited by a paper entitled, 'Rapidly Fatal Puerperal Peritonitis. Atmospheric Constitution of 1825-6-7-8. By Doctor Farrell.' Here it is stated, on authority, I presume, not to be doubted, that one practitioner in London lost 7 patients in a few weeks, and another 4 within one month, all by similar disease; and that one consulting practitioner saw 13 cases, 11 of which died. In hospital practice," continues Dr. J. Clarke, "such events do occasionally occur; but in this city (Dublin), I can speak confidently, since the year 1781 they are unknown to us among the upper ranks of society." Sir Charles Mansfield Clarke, in his answer, March 10, 1829, writes :—"But I should not say that my own experience in consultation cases agrees with the statement to which you allude as to the mortality amongst lying-in women in London during the last four years; but as I believe one form of peritonitis is infectious, and as this form admits little in the way of remedy, several such cases may occur in the practice of one man, whilst another in the same district may be entirely exempt from them. Many examples of this kind have come within my knowledge. I wish it were in my power

to say that I knew anything about the run of this disease; but every year's experience convinces me of the intractable nature of this complaint, and of the entire inadequacy of any mode of treatment with which I am acquainted in curing it. I do not here speak of a pure acute peritonitis, but of that disorder of which we spoke together when I had the pleasure of meeting you in London."

I have been induced to give these extracts from the correspondence of these great obstetric physicians, not merely as elucidating Sir Charles M. Clarke's views of the fatality of what he considered the true puerperal fever, but from their bearing upon questions of contagion, as well as from Dr. Joseph Clarke's views of the hospital being its habitat, and its spread in private practice evidently having been there traced to contagion not epidemic influences. But the really valuable part of the correspondence, and that to which I wish especially to draw the attention of the Society is, the statement of Dr. Joseph Clarke, who was as well the ablest obstetrical physician of his day, or perhaps of any day, and the physician of the largest consultation practice for nearly sixty years in Dublin, "that between the years 1781 and 1829 he could speak confidently they (puerperal fever cases) are unknown to us among the upper classes of society, although in hospital practice such cases do occasionally occur."

Dr. Stokes asks—"Is simple pregnancy the cause or the source of the development of a malignant poison? Who would believe that?" And continues—"The condition of a parturient woman had been compared to that of a person after a surgical operation. The analogy is by no means perfect." In the one case he says—"The patient is the subject of a natural process, a process which the Almighty intended to be perfectly consistent with health and with life. In the other case, the patient, who is subjected to a great surgical operation, is the subject of an entirely different process. The analogy is good in a mechanical, but it certainly fails in a physiological point of view."

Now, that there is malignant poison developed in pregnant females evidence has been adduced over and over in the course of this discussion, and that it is malignant, Dr. Stokes' recorded opinion of the invariable fatality of puerperal fever is the best proof. That pregnancy, or more properly, parturition, is the cause or the source of it is evident from its being a disease "*sui generis*" only to be met with in pregnant women. As to the analogy between parturition and a surgical operation, denied by Dr. Stokes, I cannot conceive any two processes, not identical, between which a fairer or a stronger analogy could be drawn. The reception which this analogy has met from every speaker in this debate who has alluded to traumatic metria, is remarkable.

Dr. Stokes' denial of this analogy, and his insisting upon labour being "a natural process which the Almighty intended to be perfectly

consistent with health and life," is strangely at variance with our finding this eminent and philanthropic physician in the ranks of those who deny the necessity of effecting changes in our great hospitals, which are calculated and directed towards lessening a death-rate occurring in them "amongst those subjects of a natural process, a process which the Almighty intended to be perfectly consistent with health and life," in the proportion of one out of every thirty-one delivered. Surely if it be a natural process, one out of thirty-one should not die in it; and if such a mortality occur, in what Dr. Stokes denominates and takes so much trouble to prove is a natural process, then it is his duty to detect the cause of this great mortality in a natural process intended by the Almighty to be consistent with life and health, and to remedy it in the manner that experience has already proved it can alone be done.

Had Dr. Stokes ever once witnessed a labour which, unfortunately for the cause of the parturient now at issue, he says he has not, I doubt not he would have detected the analogue he denies, and changed his mind upon the true nature of the process of labour. He would have then felt the full force of those words—"in sorrow and anguish shall thou bring forth"—which appear to have escaped his recollection in reasoning out this question. He would have borne in mind that labours are divided in our school-books into natural, preternatural, and tedious. He would have had before him the fact that in the most rapid cases it is often the most dangerous; whilst in the tedious cases it is unendurable. That the shock, *physiologically* speaking, inflicted upon the female constitution is quite as great as that produced by a capital operation, whilst the amount of pain is infinitely greater. Again, the delay in the suffering and the amount of pain is such that, were it to continue uninterrupted, and without the intervals of freedom, no female could survive it. If he will add to all this that, in a very considerable proportion of cases, where manual interference is had recourse to—when the labour is too rapid or very tedious—lesions of a serious, sometimes of necessarily fatal nature occur. Had he, in fact, witnessed one labour, I doubt not he would have changed his mind upon the perfectly natural process he makes so light of, as well as of the intentions of the Deity in regard to it.

Dr. Stokes, on the authority of a friend, drew the attention of the Society to a very remarkable fact. Puerperal fever is the endemic fever of Ballarat, prevailing throughout the scattered cottages of the country to a most alarming extent, and, in fact, being the only prevalent fever of that settlement.

If such be the case, then, we must look upon it as unique in its occurrence, and constituting an exception to the known laws which regulate this disease. At present, we should be only justified on Mill's principle applicable to exceptions. First, to make quite sure of the fact,

and then to investigate the reasons and causes which are sure to exist, if we can detect them, why in this case there should be such an exception to the generally prevalent laws with regard to it. In illustration of the necessity of the first precaution, we may mention its having been asserted that puerperal fever had not shown itself in Kilkenny for twenty years, or within the memory of any existing man. That Kilkenny has not at least been free from it, is conclusive, from the answer of Dr. Lawlor, who practised for many years in that city. He has answered my inquiries on this subject to the effect that he and the late Dr. Cane attended a fatal case in private practice, in the year 1847. Under no circumstances should we admit such an exceptional case to influence us in the results to be drawn with regard to the vital question of isolation in our present discussion.

What would the learned Professor or this Society think of my logic and conclusiveness in reasoning, if I told him that there was a district of the Ojibaway country in which the Indian tribes were never free from small-pox, and then argued on this exception that all children attacked with pyrexia in London and Paris, and the other great cities, might be moved at once into the small-pox hospitals with perfect safety, because they were equally liable to have their pyrexia eventuate in small-pox outside as inside the small-pox hospital? Now, I admit, this sounds very absurd, but to my mind not one particle more so than any argument based upon Ballarat.

We may remark, in passing, that although in the case of poisoning by animal and vegetable exhalations, we have generally the advantage of the physical safeguard afforded by the sense of smell to assist us in their detection and removal, in the group of diseases generated and communicated by crowding we are deprived of this safeguard. All we can usually detect is a close atmosphere and certain exhalations, as in the lying-in hospital wards, that prevail, whether disease be present or not. This want of notice is unfortunate in this most deadly class of disease; for although we have known of cases in which physicians have spoken of experiencing a certain whiff from the breath of a patient in typhus fever, which they felt at the moment was infecting them, yet this is so rare as not to prove generally valuable as a precautionary symptom. On the contrary, both in the typhoid and ague groups of fever the odour is often a great protection. This is especially observable in tropical climates, where these diseases, and particularly the latter, so much prevail. I was so struck with a communication I had from my son of his experiences on this subject that I am tempted to give it to you. He says:—"In India, we are pretty well broken to bad smells. A dead elephant, no uncommon nuisance to be encountered in a campaign or forced march, is very bad. I have seen a column go round half a mile to avoid one. But, for a good sustained smell, I know of nothing that

beats that of decaying vegetation in the Indian jungle. The first time I experienced this was on the march to Bhootan. We were in the Assam country, just on the edge of the Dewars; the weather was very hot; and at first I thought that the offensive odours resulted from the effects of the heat on the large body of men, so I separated myself from the column and went off alone, when, instead of abating, the smell grew rather worse, and I began to think it must be carrying it with me; but on examining, I found that the stench—for it was nothing else—arose solely from the masses of decaying vegetable matter that filled the jungle. Within a few days the men began to suffer from fever, and within one month from the date at which I first perceived the smell, out of our officers, 26 *in number, only two others and myself had escaped fever;* and among the men the rate of sickness was 60 *per cent.*"

Mr. President, you have thought it your duty, whilst in occupation of the chair of this Society, to throw yourself into this debate, as you have a perfect right to do; but whilst in dealing out criticism as a member of this Society, you must recollect that you have stepped down to the level of your compeers. You, sir, like them, must be satisfied, therefore, to accept such retort to your observations as they seem to require at my hands. Before dealing with your criticism, sir, you will permit me respectfully to refer to certain preliminary observations made by you in answer to my remonstrance upon the highly inconvenient, if not objectionable, manner of conducting this debate. I already declared that my object in producing my paper at this Society, in place of simply publishing it, was to secure full discussion. But, sir, "Il est a des fagots et des fagots." Can you for a moment imagine that I ever contemplated that I should have been called upon to sit out—I thank you for the word—the "crambe repetita" objections of fourteen speakers of written treatises, each extending to an unlimited period of time, some to an hour and a half, and one "horresco referrens" to one hour and forty minutes? Can it be supposed that in my most sanguine aspirations for fame, I could ever have anticipated that the rules which bind all societies of limiting the speakers in a debate to reasonable and practicable allowance of time should have been totally disregarded in this instance? And that I should have been martyrized to the extent I have been, and obliged to sit out in silence the same, I shall not call them futile objections or platitudes; but the freedom of debate enables me to denominate them as the same erroneous statements, the same unworthy imputations iterated and reiterated, "iterum iterumque," by fourteen learned but verbose doctors.

No, sir, such a trial was beyond human endurance, and I remonstrated, but alas without effect, and I was eventually obliged to discontinue my attendance. If the Council of your Society determined upon adopting such a system of *discussion*, if so you designate the preparation and

utterance of fourteen elaborate papers in answer to my one, they should, at least, have informed me of their intention as well as the fourteen learned doctors, and I should have well considered whether I should have submitted myself to such an infliction. Does any gentleman mean to tell me that he would have gone through these fifteen papers, my own included, in his own study, were he a free will agent, and not caught in such a trap as I was? I therefore maintain, sir, that I was justified in remonstrating, as I did, against the novel system of debate by bill and answer of unlimited length, adopted especially in my case, and that I was not only justified in doing so, but as the founder of this Society, in warning you, that although it may survive one such *irregularity*—that is your own language, sir, not mine, and a very appropriate word it was—although, as I say, it may survive one such *irregularity*, repetitions would prove its destruction.

Sir, I regret to be obliged to state that, exactly upon the same grounds, I have felt it my duty to abstain from noticing all those gentlemen who have commented on the few observations made upon treatment in my paper: I feel constrained to abstain from noticing any observation in that branch of your criticisms.

Respect for the Society over which you preside, and regard for yourself, have determined me not to apply the same rule to your other criticisms.

We shall therefore, with your permission, pass to the second point, as you term it, in which you put the word "pest-houses" into my mouth as applied again to the great lying-in hospitals. Sir, I tell you the word is yours, not mine. And sitting as you do in *quasi* judgment in that chair, it is a serious thing for you to use such language lightly, to whomsoever imputed. Be careful, sir, I beseech you, how you repeat such a phrase. These epithets stick. I am the more induced to offer you this advice, unasked, from your conveying to me that you are still open to conviction, and not a member of that overwhelming "organization" alluded to by the naughty press, "got up to stifle free discussion on a great sanitary question." I admire your defence of the speakers upon one side of this question. It is worthy of all praise, the breaking a lance with the all-powerful press, if done in the right way. But, sir, I confess I think you, the chairman of this Society, whose duty it was to secure free discussion, and to act the even-handed judge upon the occasion, ought, at least, to defend the Society at large, and not merely one side of it engaged in a discussion. And you must excuse me, a sa well-wisher to the Society, when I say you have gone further by your defence to convict the Society, or a portion of it, of the charge brought against it, than all the straightforward criticism of the press has done. Sir, it may appear ungracious my being thus explicit upon this subject, more especially when my eye is attracted a little lower in your paper to the graceful words applied to me, in which you conclude your remarks upon the criticism of the press.

Sir, much as I feel obliged and gratified at your good opinion, you cannot misunderstand me when I say I decline to accept a compliment, paid even by you, to my self-apparent objects and motives at the expense of my judgment, my discretion, capacity, and duty.

But, sir, let us deal briefly with your reasoning upon my figures. These you call in question by a general denial as regards the Dublin Lying-in Hospital, without any grounds, and by a special denial as regards the Coombe Hospital, of which you have been for twenty-eight years physician, upon the following grounds.

You deny my calculation that 3 out of 4 die in the Coombe Lying-in Hospital "that should not," or in other words, from "preventable causes," and say certainly my calculation cannot be based on the return made to me of the Coombe Hospital for the last seven years; and you add that the tabular statement subsequently furnished by Dr. Kidd in no wise corroborates it. Now, with Dr. Kidd's or any subsequent returns or with how they may vary or how affect the death-rates I have nothing to do. I have only to deal with the authentic paper upon which I based my calculations, furnished me by the Registrar of the Coombe Hospital. This places the death-rate of the Coombe Hospital for the last 7 years at 1 in 72·25. Now, sir, the mean of the three small hospitals with which the Coombe was compared, gave a death-rate of 1 in 282¾. Four times 72 make 282, therefore if 1 in 282¾ was taken as the basis of our calculation of the average proportion of inevitable deaths in labour, we were justified in treating this great sanitary question, to state that 3 out of 4 of the deaths that occurred in the Coombe were unnecessary, in other words, if only one constituted the necessary or inevitable proportion of deaths *ceteris paribus*, that the 3 others were in excess evitable and consequently unnecessary, and if this be so, you, sir, by a parity of reasoning were not justified in your special denial.

You will, sir, I trust, excuse my declining to take in detail your explanations or ideas showing why the deaths should be taken, in the case of the Coombe, out of the general category, or admitting that your cases, many of which, in all likelihood, died of metria, should be classed under different heads, and not metria. The forceps may be used, version may be necessary, that dire necessity—craniotomy, or cephalotripsy may occur, and yet it is but too familiar to us to know that all these cases may die of metria. Thereupon the three pages in which you have amplified upon the distinctions of the state that led, possibly to metria, in its different modifications, is not germane to our discussion. In entering upon this inquiry I distinctly disclaimed attempting to base my caculations upon the numbers of cases of metria. I found the difficulties of arriving at what was metria so great that I determined to rest upon the general mortality as the only true basis of calculation. It is a very simple matter for you, sir, or any other inquirer, to arrive at your

own conclusions upon the proportions of metria and accidents in childbirth, but as it has nothing to do with the calculation at issue, I must simply reiterate that Dr. Ringland has failed to shake my statement, based on the authority of his own registrar's return of the Coombe death-rate for the seven years furnished to me, which leaves me no alternative but to arrive at the conclusion that 3 out of 4 evitable deaths occur in the Coombe Lying-in Hospital.

With Dr. Ringland's third point I quite agree. He says, and saves me much trouble by saying manfully, when dealing with the alteration of our hospitals, to prevent contagion—"I believe the true prevention is isolation." Had he stopped here I would have accepted his opinion as conclusive and valuable, but unfortunately he adds—"instant and complete isolation of the infected from the healthy." That is, he is satisfied that the true prevention is isolation, but he would not insist upon this until probably the mischief was done. But he corroborates this opinion by begging the question as to the time contagion develops itself, which he has fixed at some convenient season after the attack that admits of the removal of the patient; yet, on the other hand, you have Dr. M'Clintock and others assuring you that the attack sets in at any period, and spreads at the earliest period after, or almost simultaneous with, its own commencement; and you have me, as well as Dr. Stokes and others, telling you that it occurs before delivery.

What becomes, then, of the statement that "contagion does not exist in the incipience of the attack?" Sir, it does not require to prove even this, as the fact is, in the cases where the disease shows itself, it don't require immediate contact. The poison pervades the hospital. The hospital is its habitat, and be the patient removed or not the taint spreads, when it generates, haunts, and destroys. You, Mr. President, give us one crumb of comfort in the debate. You, like Dr. Kidd, announce, and speak with confidence of a plan to be adopted in the Coombe Hospital, in future, of keeping an isolated building, entered by a separate stair, for metria patients, to be removed to when attacked. I accept this announcement as that of a step in the right direction. But I could have wished you had been able to announce a step farther, and that isolation in its true sense had been determined upon in your new structure.

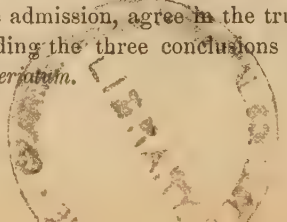
I may mention before passing from this subject, that a very valuable testimony is afforded on the subject of isolation in a report furnished by Dr. Graily Hewit, of the British Lying-in Hospital, in which he remarks:—"During the early part of the year 1862, and before I had taken, in conjunction with Dr. Murray, charge of the in-patients of the hospital, several patients were the subject of puerperal fever, and four of them died; two others were discharged in a very weak state; and the hospital was for a time closed. As soon after taking charge of the

in-patients as circumstances admitted, steps were taken by Dr. Murray and myself to isolate the patients to a greater extent than before. The number of beds in each ward was reduced from six to four, and the communication between the wards as much as possible cut off."

Let us now see what was the result of the partial approximation to isolation, for Dr. Hewit does not lead us to suppose it was quite perfect. The mortality in the four years previously, in which time there were 428 deliveries, amounted to 7, that is, to 1 in 61; whereas in the next four years there were 10 deaths in 761 deliveries, which was only 1 in 75, and this although the number of deliveries was nearly doubled. This calculation I look upon, therefore, as extremely valuable, as showing the advantage to be gained by even an approximation to isolation, whilst it also shows the check to the benefit that would in all likelihood have been derived from it had not the crowding on admission of an increased number of patients counteracted the advantage of the attempt at isolation. It is thus very valuable in showing, that partial and not complete isolation will only disappoint our expections of preventing the disease.

I feel convinced, sir, that you and this Society will agree that I have occupied sufficiently your time in dealing with individual criticism, and that you will be prepared, at this stage of our discussion, to expect me to deal with the subject generally. We shall therefore ascend *de particulare ad generalem.* In doing this I should mention that I attempted to draw out a tabulated calculation of the opposition and support given by the members who have spoken on the debate, and I find, as nearly as I can calculate, that in dealing with my thirteen propositions, the 16 speakers expressed 76 opinions. That would arrive at very nearly an average of 5 opinions each proposition. Of these 53 supported the propositions and 23 opposed them, or supported their converse. But this gives us little information of how far they supported my views, or what views exactly they supported and what they opposed. I am sure I shall be excused in making this analysis when I say, although many of my critics attacked me and the proposed changes I advocated, most of them were fain to admit the basis upon which my arguments were fixed, whilst they objected to my conclusions and remedies for grievances as patent as the sun at noonday.

We shall give one example of this which will apply equally to several other of my propositions. Ten gentlemen freely admitted metria as contagious. But, sir, it must result from the simplest process of reasoning that every gentleman who has admitted the contagious nature of metria, in however small a degree, must, as a matter of course, and, as a consequence of the admission, agree in the truth of twelve of my thirteen propositions, including the three conclusions resulting from them. Let us just test them *seriatim.*



Proposition No. 6.—It is contagious consequently.	Proposition No. 1.—It is due to dissemination of poison.
It is contagious consequently.	Proposition No. 2.—It is absorbed into the system either of the generator or of another parturient exposed to its influence.
It is contagious consequently.	Propositition No. 3.—The generation or spread must be in proportion to the number of parturients exposed to the poison.
It is contagious consequently.	Proposition No. 4.—In lying-in hospitals where large numbers of patients are delivered under the same roof (if it exists) it *must* find its habitat; and we may add prove destructive in proportion.
It is contagious consequently.	Proposition No. 6.—It follows in steps of certain practitioners, whilst others are totally free from it in the same locality.
It is contagious consequently.	Proposition No. 7.—It is endemic, or confined to the locality where the contagion prevails.
It is contagious consequently.	Proposition No. 8.—It is not only confined to a given hospital, but is observed to haunt certain wards of that hospital in which it has its special habitat.
It is contagious consequently.	Proposition No. 9.—It occurs comparatively rarely among women confined in their own homes, where they are only exposed to the risk of self-contamination or other zymotic diseases which it is observed to succeed.
It is contagious consequently.	Proposition No. 10.—It is not observed to occur in small lying-in hospitals, or cottages where one or two patients cohabit, and the elements of its generation and spread don't exist.
It is contagious consequently.	Proposition No. 11.—We are causing the death by metria of a number of patients by continuing the gregarious system of large lying-in hospitals, who would escape under a system of segregation or isolation.
It is contagious consequently.	Proposition No. 12.—If lying-in hospitals are to be continued for the cure of patients and as schools of instruction, which they undoubtedly ought to be, they can only be so continued by substituting isolated cottages or pavilion hospitals with one or two beds in each isolated room.
It is contagious consequently.	Proposition No. 13.—With our present knowledge the conclusion is inevitable, that the mortality among parturient women would be greatly lessened by an alteration in construction and an arrangement of lying-in hospitals which would secure segregation and prevent congregation.

But, sir, the contagionists must go much further. They must use their reason, their experience, and their common sense, in testing the value of any apparently adverse statistics—I mean statistics that appear at variance with the principles that apply in all other contagious

diseases; and if they find that any collation or coaptation of figures eventuated in bringing out results at variance with reason, experience, and common sense, and with those laws that apply immutably in all diseases of the zymotic and contagious types, then they must "hold fast by sound words," and reject the figures as either exceptions which prove the rule, the explanation of which they are ignorant, or fallacies beyond their detection. Such are the figures, from what quarter soever they may come, that would go to prove that contagious diseases will not spread, *ceteris paribus*, in a direct ratio to the number exposed to the contagious influence. This, supplementing the effect of crowded houses in producing disease, is, in fact, the gist of the whole question lying at the root of this discussion. If a disease be contagious it can only spread by exposing a susceptible party to the influence of the contagion. If two be exposed the risk is doubled; four, quadrupled; and so on in an arithmetical ratio. Exactly in the same ratio is the risk in the spread of metria in the larger in comparison to the smaller cottage hospitals, arguments, elaborate treatises, and statistics *notwithstanding*.

Most of my commentators have started on a wrong basis in this discussion.

They have dealt with me as an antagonist in a matter in which there can be, or at least there ought not to be, no antagonism.

We are here as a scientific body, discussing an abstract sanitary question.

I affirm that there is a preventable loss of human life from a particular disease.

I suggest the method of lowering this mortality, and how am I met?

By a denial that the mortality exists.

I go into my proofs, and my facts are not disproved.

But a standard of mortality is set up different from that selected by me.

The invariable rule hitherto with sanitarians, whose ostensible motive is to improve the death-rates, is to place the highest standard before them, and direct their efforts to accomplish this.

The rule adopted by my commentators has been to select the lowest standard, and to level down to this.

What should we think of the physicians of Newcastle-upon-Tyne if, in answer to Dr. Farr's strictures upon the unhealthiness of their town, and his remonstrances upon their high death-rate, 37 in 1,000, they, in place of directing their attention to ascertain the causes and removal of their unenviable notoriety, denied its existence, and then set to work to prove their case by a comparison with the mortality of Manchester, which is equally notorious. Not Hull, in which the mortality amounts to only 23 in 1,000.

Yet this is exactly what has been done by my commentators, who

have questioned my statistics, and adduced counter statistics showing a higher death-rate in bar of correcting the unnecessary death-rate that I have pointed out. A large proportion of this debate has been directed to this futile, this irrelevant object. Our attention has been misdirected. The "*tu quoque*" is no justification. The "I am no worse than my neighbour" is no excuse.

A grievous mortality exists in these great hospitals. I care not *where else* it exists if I can prove to you that it does not exist *elsewhere;* if I can show you that human life is preserved in the proportion of 2 to 1 or 20 to 1 in a different place by a different plan from that adopted in the hospitals or places where the mortality exists; and that such a plan is available and practicable, or even not impossible in these also, my case is made. Now, sir, I have proved all this, and nothing remains to my opponents but to withdraw an antagonism that has already, I regret to say it, attracted to this discussion an unenviable notoriety. I have proved, sir, not only that the high rate of mortality from metria exists in our hospital, but that it has done so with little intermission from its foundation to the present day. What says a resolution of the governors' on the subject, passed on the 17th June, 1815, fifty-three years ago:—"A crowded state of the wards appears to the committee rather incompatible with the health of the patients, as under such circumstances the hospital will thereby remain liable to a recurrence of *that infectious and fatal fever which has on several occasions been so severely felt.*"

What says the first authority of the present day upon zymotic diseases, Dr. Farr, Registrar-General, in his Report on Causes of Death in 1867, fifty years later, on this subject:—"The mere aggregation of people together in close apartments *generates* or *diffuses* the zymotic matter. Thus, *place lying-in women in close proximity* to each other, or mix them up with the patients of a general hospital, and they *died of puerperal fever.* Place many wounded men in a ward where cleanliness is neglected, and erysipelas, pyemia, gangrene spring up; imprison men within narrow walls, or crowd them in rooms, and typhus breaks out. The general and special hospitals of the country have been until quite recently erected without any special reference to the dangers accruing from the assemblage of great masses of sick people within the walls of one building, so that the efforts of the most skilful medical officers are frequently defeated." Again, Dr. Farr says—"The mere *accumulation* of masses of living people within narrow limits either *generates* or *insures* the diffusion of epidemic disease." Again, page 220—"The suppression of the generating beds of disease in unhealthy populations can scarcely fail to be efficacious. To suppress plague, suppress the wretched sanitary condition of Egypt; to suppress yellow fever, go to St. Thomas, New Orleans, and its other feeding grounds; to put a stop to epidemic outbreaks of cholera, cleanse the waters of India, and improve the condition

of the population; to extinguish entire fever and typhus in our cities, extinguish the rookeries." And he might have added, to lessen the mortality by metria, if not to "get rid of the generating beds of this disease," reform the lying-in hospitals.

But to return to the attempts made to involve me in a discussion upon a collection of statistical tables, in which this higher standard of death-rates is dwelt upon as that to be aimed at, and with which we are called upon to rest satisfied. I once for all tell my commentators, that I shall fall into no such palpable trap; nor be led away from the true issue by any such—excuse the word, clap-trap.

I, therefore, disregard, with the utmost self-complacency, all their broad-sheets and narrow-sheets, teeming with figures and quantities, which a high death-rate, as the aim for us to attain. Nay, I will, if it be any comfort to them for treating the ponderous result of their labours cavalierly, I will admit them all *in globo*, and that without examination, as not germane to the question at issue, and only calculated to draw us aside from our great object, "how to lessen the mortality from metria." I want not to be reconciled to it, on the contrary, my object in coming before this Society, amongst whom I see so many of my alumni, is that of the Carthegenian of old, I would urge them to swear with me eternal hostility to our common enemy, metria, and, if possible, to unite the Knights Hospitallers, who have so strangely mistaken their antagonist in this discussion, to make common cause with me in our efforts at victory.

Sir, I may say generally that my commentators in this controversy have supported every one of my positions, although they may have cavilled at the extent to which I have carried them. When I say cavil, I mean that they have taken objections on points, and failed to grapple with the broad principles. They have attempted to draw the attention of the Society off from the discussion of a great question, boldly, and, I admit, as my friend Dr. Beatty has said, startlingly, propounded, by involving it in a mass of what we shall term discrepancies of detail. They imagine that by showing a want of harmony and exact correspondence in all the collections of statistics they could scrape together, that they can break down the notorious facts which I arrayed before this assembly on introducing this subject—an array of facts, proverbial, and confirmed by the history of the great institutions of Europe, from their foundation to the present hour. Can they deny my tables in which the mortality in Paris, St. Petersburgh, Vienna, Dublin, London, York, and Glasgow Hospitals are given, and the combined mortality of which is 1 in 44; whilst the death rate ranges even so high as 1 in 18½ in Paris? Can they deny the average death-rate of the Dublin Lying-in Hospital, standing for the last 15 years at 31½? Can they deny that for four years of Dr. Collins's mastership, it stood at 1 in 186, showing what the

true death-rate is—accidents of labour and diseases included, metria excluded? Can they deny that the death-rate of the South Union Workhouse Cottage Lying-in Hospitals had only amounted during the last four years to 1 in 238, and that no metria showed itself there during that time? Can they deny the report of the death-rates as given in Drs. Thomas and John Beatty's private practice, as furnished by Dr. Thomas Beatty himself, as 1 in 256? But, above all, can they deny, or, rather, have they not failed deplorably in their attempt to disprove, the mortality of the three cottage hospitals, Waterford, Limerick, and New Ross (to which we may now add Killarney), as standing at 1 in 282?

Sir, they cannot deny. They cannot vitiate. They cannot shake in the slightest degree one of these reliable and unanswerable bases of the reasoning upon which my conclusions are established. My conclusions, my deductions, and my propositions are the necessary sequence of their establishment. Sir, I am certain, and maintain without the fear of contradiction, that the effect of the ordeal my paper has undergone has been its entire confirmation.

As to the discrepant statistics adduced by them—their very discrepancy may be said to go a long way in proving the accuracy of mine. The laws of evidence have long established the fact, that want of exact harmony or correspondence in different testifiers is esteemed rather a corroboration than a denial of truth—a principle that is adduced as well by our ablest theologists in proof of the inspiration of Holy Writ, as by our petty sessions attorneys in the establishment of an alibi. But, sir, it was a primary axiom in my days of pupilage, that "one affirmative proof overturned a host of negative evidence." If the combination of philosophers who have been dealing for months with my propositions cannot reverse this axiom, then I submit that my propositions, and especially that one which has been so much cavilled at—the proportion of deaths to numbers and crowding—has been proved, aye, over and over again, by every great Lying-in Hospital in Europe. Why, sir, my first impression on hearing it objected to was one of what I might describe provoked amusement, that any man could be found so innocent, or so obstinate, as to deny such a self-evident proposition. It was in this spirit, I confess, that I, upon the third night of our sederunt, called the attention of the gentleman, who has the merit of first making this great discovery, to the case of Dr. Labatt's great mortality,—the very case to which he referred as corroborating the reverse. His mortality I repeat—1 in 63 in 1818—the year of the greatest number ever in the hospital. I showed him that the next year the habitat was complete, and the mortality rose to 1 in 33, although the deliveries were reduced 342; that it continued at 1 in 35 the year after, when the deliveries were again raised; and it gradually wore itself out in the course of the next two years, when the crowding was again permitted, and that in 1823 it again rose to 1 in 44.

I was in the hopes, that this statement of figures would have set the fallacy of confounding cause and effect, in this case at rest for ever. I imagined in my simplicity that ascribing the greater rate of mortality to the diminished number of patients, in place of the diminished number of patients to the greater mortality, was at an end, and that so preposterous a proposition should not be repeated. I have, therefore, to remind you that this step was taken to relieve the hospital from continuing to be the habitat of the poison. I am fully aware sir, that I am repeating myself, but I am doing so designedly, because, to my astonishment, the fallacy has been repeated, and dwelt upon again and again in this discussion, notwithstanding my exposure of it. Dr. M'Clintock repeated it; Dr. Beatty repeated it; and a host of other doctors, too numerous to mention, repeated it, and nothing is left to me but to repeat the refutation of it.

I purposely avoided dwelling much upon treatment in my former paper, my object being to grapple with the true nature, causes, and means of prevention of metria. I thought, however, that the profession might have reason to condemn me if, in a treatise of this nature, I had entirely omitted to allude to it. My reticence in this respect has not relieved me from the usual fate of "mine enemy who writes a book." My treatment has, in fact, been attacked as unmeasurably as my other views. I admit it is equally open to candid criticism, if this had been extended to it; but when the reverse has been the case, as it has in every instance in which my treatment has been called in question, I shall give these attacks but one general answer, namely, that when garbled extracts are taken and misrepresentations, proved by the context, put forward, I deem it both unnecessary and unbecoming to notice them, and I feel called upon to omit all mention of the critic and his objections—a practice that I also adopt when commentators outstep the fair bounds of criticism and descend to personalities.

By this simple plan the crime carries its punishment along with it, whilst temper and offensive personalities are prevented displaying themselves on my part, in what I esteem as a purely scientific inquiry, and one into which personal feeling should not be permitted for one moment to enter.

Why, sir, if another night had been added to the debate, it is not impossible that the conclusion might have been arrived at by some latent essayists of the Knights Hospitaller ranks—some of the more courageous and *advanced* advocates of this *retrograde* movement in the nineteenth century—that the true cure and prevention of metria would be best accomplished by crowding all our parturient populations into large hospitals, and for this purpose not a moment should be lost in enlarging our existing lying-in institutions, and extending them upon the present principles, which have been found so satisfactory.

Some general objections have been raised to remodelling our hospitals,

based upon such grounds as cost, expense, disinclination to change, architectural beauty, and soforth. They should be left as they are. The Deity has ordained that people should die epidemically; for his own wise purposes epidemics have always existed. Now, sir, I shall not insult your understanding by combating such objections. Most of them were used over and over again against Jenner; and although the establishment of vaccination was retarded by them, it was not prevented. Could we now calculate the amount of human life and human misery saved by the introduction of vaccination, and of that loss and misery caused by *their* opposition to it, this calculation might be turned to good account in the present discussion.

Sir, already has this discussion produced good fruit. The strong and universal opinion, I may say, pronounced upon the contagious nature of the disease, and the attention that has been more especially called to its spread by means of the indiscriminate mixture of the male pupils with the patients after attending autopsies, has, I understand, elicited an order precluding this practice, and thus for so far our common object has been gained—namely, the taking steps to endeavour to lessen the mortality by metria and other zymotic diseases. As I doubt not these precautionary measures will extend to other diseases communicable by contagion, the knight's service rendered by several of my critics upon this point reconciles me to much of their shortcomings in other branches of this inquiry. And if we applied the same principles of preventing the spread of contagion to the other equally important and equally obvious means by which contagion is necessarily spread through crowding together patients similarly affected, under a common roof, there would virtually be no question at issue between us. We should all be, on many points, of the same opinion, although still, I regret to say, on one or two essentials, "as far as the poles asunder."

Sir, I might have appealed to their benevolence, love of science, and desire for improvement, as a motive for acquiescence in my views, and urged their adoption with arguments and persuasions thus adduced. But, sir, in a severe inquiry such as this, I should by such a line only load my case "with superfluous armour, that encumbers rather than defends." I might tell them that communications had reached me from various quarters, such as that I now quote, in which Dr. Handyside, the distinguished surgeon for so many years to the Edinburgh Royal Infirmary, writes :—"I take this opportunity to congratulate you on the work as to wholesome village hospital wards instead of large maternities, in which, from the reports of the journals, I see you are engaged. Your experience and position ought to carry conviction, and it surprises me much to see so many of your profession opposed to the proposal. So far as my experience as hospital surgeon led me to see, we must, in the end, be led to give up the large hospital buildings.—Edinburgh, June 10, 1869."

But I shall not detain you with these from the result arrived at on the discussion of this subject in the Surgical Society of Paris, in which Trêlat, Le Fort, and Jardier were the principal speakers.—See the *Gazette des Hôpitaux*, No. 67, 1866.

The following conclusions were arrived at:—

1. It has been proved by statistics that the ravages of puerperal fever in lying-in hospitals are greater now than formerly. This can only be referred to the hospital atmosphere; therefore, the infirmaries and hospitals should be reduced in extent, and assistance provided for the poor at their own houses.

2. Puerperal fever is infectious; and, therefore, hospitals conducted on the best principles may become the seat of great calamities.

3. Besides the usual sanitary measures which are recommended for hospitals, special precautions should be observed in lying-in institutions.

4. To avoid importation of disease, strict cleanliness should be observed. Empty wards should be thoroughly cleaned, not only the walls white-washed, but the beds purified, &c.

5. To avoid the spread of the disease the healthy should be removed from the ward, where any have been attacked, to small rooms for one, or at most four beds.

6. The attacked should be removed to a separate building.

7. If, nevertheless, the remaining females get the fever, the whole building must be emptied.

8. In cities where lying-in hospitals cannot be dispensed with, they must be small.

Since the above conclusions were arrived at by the ablest of our Parisian confreres, as we are informed by Oppert, "The administration in Paris have had the wards divided by double glass partitions; but, recently, they have gone farther, and recommended single bed-rooms."

This is striking at the root of the evil; and we trust that they will speedily go farther still, and have these bedrooms totally isolated and devoid of all closed communications, capable of sustaining and communicating a poisoned atmosphere from the sick to the healthy. Anything short of this must prove inefficient, and end only in disappointment.

You see then, sir, whilst we are discussing this question, it is deciding itself. Nay, sir, it has decided itself; and decided itself, gentlemen, not as should have been the case, *with* the strenuous assistance of those members of the obstetrical branch of the profession who have ranged themselves to oppose my feeble efforts (to the knife), as they say themselves, but in the teeth of their resistance. Sir, the enlightened press of England, of Ireland, and of Scotland, has decided this question. It is an accomplished fact. Let the recusants hasten to join in our efforts, or they will rest under the inglorious conviction that theyhave wedded themselves not merely to an unsuccessful cause, but to one that is being stamped with

the just reprehension of the medical and general press throughout this country. That great organ, that sits in judgment upon men's reasoning, their motives, and their actions, and draws its conclusions for the universal benefit—and what does it say, gentlemen, upon the merits of this discussion? Do not be alarmed. I am not going to accumulate and reproduce the numerous articles that have dealt with it. I shall merely give you the brief view of one of the ablest of the London journals, the *Echo*, upon your own case in its article headed "The Harvest of Death, June 10, 1869." "It is worthy of note," says the *Echo*, "that the idea of maternity hospitals, which were once so popular, *has been exploded by scientific* experience. It has been found that such institutions are merely nurseries for puerperal fever." Now, gentlemen, what say you to this? The press of England have arrived, without the assistance and in the teeth of your opposition, at this conclusion, that maternity hospitals are the nurseries or habitat of puerperal fever. But it is not done yet. Take to heart, I beseech you, the next paragraph of the *Echo*, and mend your hand in time. This, mark me, is the *Echo's* language, not mine. But here it is:—"The best hope of mitigating the danger under this cause (that is the cause of continuing the maternity hospitals as nurseries for puerperal fever) is derived from the gradual but steady increase of medical knowledge, and the extinction of that time-honoured institution personified in 'Sairy Gamp.'" But now for the "keenest cut of all." "A new race of Gamps, intelligent and instructed, is one of the greatest social needs of our time."

Sir, in contrast to this view of the *Echo*, I cannot deny you the quotation of a private letter bearing upon the responsibility that rests with our profession in using their best endeavours in the prevention, as they do in the cure of disease. It was an acknowledgment written to a medical friend on receipt of his sanitary pamphlet lately published.

"My Dear,—Many thanks for your pamphlet. I am glad to see that you feel a physician's duty extends beyond the cure of disease, and that the public have a right to look to him as well for its prevention. If our profession recognized and acted on this principle, generally, it would exercise a position, and earn an influence in the State commensurate with its true value. But until it does so we must not be astonished that they are treated as cyphers, not to say mere craftsmen by the State; when they rest satisfied with doing mere craftsmen's—I was going to say tinker's—work. I am glad to see, however, that you are an exception to this criticism.—Yours faithfully."

This, I think, requires no comment.

It would be amusing to reproduce some of the absurd accusations and imputed evil intentions with which I have been charged in this discussion. Mr. Sandham Symes, for instance, deals with the subject of

architecture. At first I imagined his communication a hoax; but no, it is in too serious a strain. He seems really anxious to protect the ornamental buildings of the city, and like my friend Dr. Churchill, deprecates my proceedings in so precipitous a manner in a matter in which he graphically adds, "there has been as yet but a mere trifle of experience." But of what, think you, does he accuse me? Of nothing short of proposing to remove the beautiful structure, the Dublin Lying-in Hospital, with its world-famed Rotunda, bodily, and replace it by cottage hospitals to be built upon its site, one of the worst sites that could be selected for such a purpose. Why, sir, if made, such a proposal would justify my residence in an hospital raised by the eccentric Dean in another part of our city. This gentleman might, at least, have read my paper before presuming to criticise it, in which I distinctly recommended the converting the present hospital into an institution for diseases of females (with the power of admitting children), and suggested the high ground, the most elevated part of the Rotunda Gardens, as a most suitable site for the erection of isolated cottage hospitals for the labour patients.

Another charge, or rather an insinuated imputation, was made even more absurd in its nature, in a few excited words, that said little but implied a great deal, by Dr. Sinclair, on the third night's discussion.

This gentleman introduced the name of the Prime Minister, with a suggestive insinuation that he was going to deal destructively with the hospitals of this city. No doubt, sir, in admitting that Mr. Gladstone's acquirements are as varied, and his information as extensive as conceivable, yet I may affirm, without assuming any knowledge of state secrets, or committing myself in their divulgence, that he knows just about as much of this discussion or its objects as of that important question that engrossed the attention of the three tailors of Tooley-street. Sir, I believe in my heart Mr. Gladstone's aspirations, wishes, and intentions are, as all may know who will be at the pains to inquire, to benefit the poor of this country in every manner that his enlarged mind, great experience, and devotion to sanitary improvements so peculiarly befit him to do. Sanitarians, at least, must agree that fortuately for Ireland the power, the knowledge, and the will to exercise them in the right manner, are combined in one man, at this moment, and that man is one who has taken the improvement of the sick and impotent in this country seriously to heart. Fortunately for us, as Irishmen and physicians, the practically benevolent pursuits to which Mr. Gladstone and his family have always been devoted have well prepared him to deal with the improvement of the medical charities of this country.

And still more fortunate for us in Ireland, the manner in which his efforts have been so ably seconded by the partner of his cares (a title nobly earned by her whose exertions to benefit the London charities have rendered the name of Gladstone, as that of Nightingale, a household

word with the benevolent). Sir, the medical men and citizens of Dublin have a right to know the fact, as this name has been so invidiously, and, I may add, so injudiciously introduced into a purely scientific discussion: that the same anxiety to improve and extend our Dublin charities that has so well succeeded in London, prevails in that benevolent, large-hearted mind, and unless frustrated in its exertions to accomplish its object, I doubt not the same success will attend them.

If, after the experiences, the facts, and the reasonings, and the reliable statistics that have been produced, there be still amongst my hearers and readers more than one man who denies that our large lying-in hospitals cannot, and ought not, to be improved in their rates of mortality, and that, even upon the showing of those gentlemen who have taken an adverse line in this discussion, I should like to see him and hear him say so boldly. If there be not, then there is at least one point upon which every man in this Society, and, I might add, every man who knows anything of the subject, out of it, must agree, that is, that a necessity exists for lessening the death-rates in lying-in hospitals. The means of doing that is entirely another question, and one upon which there may be as many different opinions as there are individuals present. Taking it for granted, then, that a necessity exists for an improvement, it may be further affirmed that every one of the recusants who carried, as one of them pugnaciously remarked, "war to the knife" against my inquiry—the hospital physicians and their congeners who admitted so many of my propositions, and confirmed the real basis of my arguments—and the general listeners, including the sons of Korah, who have exhibited such patience and zeal in sitting out the nine nights' discussion, I say, it may be affirmed that every one of my hearers and readers who has arrived at the above conclusion, and who, as far as in him lies, does not endeavour, from this moment, to correct it, is assisting in sustaining that death-rate. Do not deceive yourselves, gentlemen. He is not merely in the position of an accessory after the fact; he is from this day out an accessory before the fact. And what is the fact? It is the taking away of life—precious life, a gift that none of us can restore.

The law recognizes two descriptions of responsibility—that by commission and that by omission; and culpability attaches as well to the first as to the last. I now say, and say with deliberation, that every one of us who possesses the power, by act, word, or deed, of preventing the unnecessary loss of life, and who exercises his influence against that prevention, or omits to exercise for it, is morally in one or other of these positions.

Following, sir, the example of my commentators, I might have been perfectly justified in a discussion of this nature in turning to account the artifices of oratory. Like them, I might have attempted bathos and perhaps accomplished travesty.

I might have appealed to them to use their own language, not mine, not to give over "these poor women to die in the proportion of 3 to 1 that ought to die in their hospitals." I might have besought them as parents, as husbands, to throw their shield over those doomed to become motherless and bereaved.

I might have attempted to arouse their feelings by depicting the mother led in tears from her humble but happy home to the hospital to endure the pangs of labour, cheered with the confident hope of a speedy release in health and strength—a hope, alas! how bitterly to be disappointed!!!

I might harass them by attempting to depict that death-bed scene, and its consequences, with which, alas! I am but too familiar.

The strong, hopeful woman, but yesterday, grasped by the unrelenting hand of death, in a silent, lonely, strange chamber, separated from her friends, and tended by the hand of the hireling stranger, unsoothed, unsolaced by those tender attentions of her family, that deprive even death itself of half its terrors.

I might have changed the scene and depicted the shrouded victim stretched on the table of the gastly charnel-house, until her husband and surviving relatives attend to convey the remains of her they loved, and parted with so hopefully a few hours before, to her last resting place.

But who could attempt to calculate or depict the consequences of this bereavement to that sorrowing family? A father obliged to neglect his motherless family in the discharge of his daily duty. The children deprived of their guide--now Arabs on the street, swelling the huge catalogue of crime. The males, perhaps, winding up as felons—the females, as worse.

But, sir, neither my inclinations nor my respect for my audience approve of such a course, in a strictly practical inquiry, "*non tali auxilio nec defensoribus istis tempus eget.*" Nay, I might have appealed to the gentlemen who oppose this attempt at lessening the mortality by an improved arrangement of our lying-in institutions, and begged them not to rest satisfied that 1 death in 45 deliveries proves to us that matters are "tending to render the hospital as healthy as one could almost desire."

I will, however, appeal to them in the language of the patriarch pleading for the cities of the plain, and say:—Peradventure we might find a way of saving 20 more women in every thousand. Shall we not do it? Or even if, peradventure, we might save 10 more in every thousand—let us do it. Or to the recusants who object to the expense or inconvenience, with a comparatively small improvement, resulting to the hospitals, such as the modified improvements in St. Petersburgh, Vienna, and Paris, we might add—Peradventure we might even find a way of saving 5 lives out of every thousand—let us do it.

It is doubtful whether those gentlemen, my critics, whom I have not specially answered may not be more dissatisfied with my breach than my observance of this mark of respect. Should such be the case, I can only apologize to them in the words of Napoleon I. in bidding farewell to his army on leaving France for Elba:—"Soldiers, would that I could embrace you all! As this is impossible, I salute your generals."

Sir, it remains for me now merely to express my apologies for the imperfect manner in which I have executed this great, this painful duty, and to thank you for the patience with which you have borne with me.

Nine nights have been devoted to this important investigation; the amount of mortality in our great hospitals has been brought to light, and the means of preventing this have been elucidated. It rests with us now to apply our knowledge—and that without delay—for, in the words of the American poet—

"Man-like it is to fall into sin,
Fiend-like it is to dwell therein."

BOOKS RECEIVED, AUGUST, 1869.

1. Small pox and vaccination hospital. Medical report for 1868.

2. Treatise on the diseases of the ear, including the anatomy of the organ. By Anton von Tröltsch, M.D., Professor in the University of Würzburg. Translated and edited by D. B. St. John Roosa, M.A., M.D., Clinical Professor of the Diseases of the Eye and Ear in the University of New York. Second American from the fourth German edition. New York : William Wood and Co. London : Trübner and Co. 8vo, pp. 565.

3. A handbook of therapeutics. By Sydney Ringer, M.D., Professor of Therapeutics in the University College, Physician to University College Hospital. London : H. K. Lewis. 1869. 8vo, pp. 485.

4. Mind and brain, or the correlations of consciousness and organization systematically investigated and applied to philosophy, mental science, and practice. By Thomas Laycock, M.D., &c., Professor of the Practice of Medicine and of Clinical Medicine, and Lecturer on Medical Psychology and Mental Diseases in the University of Edinburgh. 2 vols. Crown 8vo, pp. 425 and 494.

5. Observations on ligature of arteries on the antiseptic system. By Joseph Lister, F.R.S., Professor of Surgery in the University of Glasgow, &c. Edinburgh : Edmonston and Douglas. 1869. Pp. 14.

6. Aperçu historique sur l'origine et les progrèss de l'ovariotome en Italie. Lettre à M. le Dr. A. A. Boinet par le Dr. D. Peruzzi, Chirurgien Primaire de Lugo. Pp. 18.

7. Cases illustrative of the antiseptic use of carbolic acid. By Joseph Bell, M.D., F.R.C.S.E., Lecturer on Surgery, Assistant Surgeon Royal Infirmary of Edinburgh. Edinburgh: Oliver and Boyd. 1869. Pp. 9.

8. Thirteenth annual report of the united lunatic asylum for the county and borough of Nottingham.

9. St. Thomas's Hospital. Statistical report. London : John Churchill and Sons. 1869. 8vo, pp. 156.

10. Operation of vesico-vaginal fistule without the aid of assistants. By Nathan Bozeman, M.D. New York : D. Appleton and Co. 1869. Pp. 26.

11. Annual report of the royal Edinburgh asylum for the insane for the year 1868. Edinburgh : Royal Asylum Press. Pp. 32.

12. Lancashire Spring Assizes. Manchester, March 18, 1869. Before Mr. Justice Lush and a special jury. Sagar *v.* the Lancashire and Yorkshire Railway Company. Manchester : Cave and Sever. Pp. 103.

13. Transactions of the Obstetrical Society of London. Vol. x., for the year 1868. London: Longmans, Green, and Co. 1869. 8vo, pp. 318.

14. De l'usage du perchlorure de fer liquide et notamment du perchlorure de fer dilue dans le croup et dans les autres formes d'angine, par le Dr. E. J. Schaller. Paris : J. B. Baillière et Fils. 8vo, pp. 89.

15. The baths and wells of Europe, their action and uses, with hints on change of air and diet cures. By John Macpherson, M.D. London : Macmillan and Co. 1869. Fcap. 8vo, pp. 336.

16. The Bahamas ; a sketch. By Surgeon-Major Bagot. London : Longmans, Green, Reader, and Dyer. Belfast: Henry Greer. 1869. 8vo, pp. 112.

17. St. Andrew's Medical Graduates' Association Transactions, 1868. Edited by Leonard W. Sedgwick, M.D., hon. secretary. London : John Churchill and and Sons. 1869. 8vo, pp. 262.

18. A course of six lectures on the chemical changes of carbon. By William Odling, M.D., F.R.S., Fullerian Professor of Chemistry Royal Institution. London : Longmans, Green, and Co. 1869. Fcap. 8vo, pp. 162.

19. On counter-irritation; a theory constructed by the deductive method of investigation. By James Ross, M.D. London : John Churchill and Sons. 1869. Pp. 44.

20. Orthopraxy ; the mechanical treatment of deformities, debilities, and deficiencies of the human frame. A manual. By Henry Heather Bigg, Assoc. Inst. C.E., Anatomical Mechanician to the Queen and Prince of Wales, the Royal Hospitals of Chelsea and Greenwich, &c. Second edition. Revised and enlarged, with 308 illustrations. London : John Churchill and Sons. 1869. Fcap. 8vo, pp. 641.

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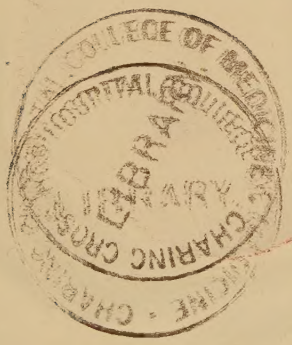
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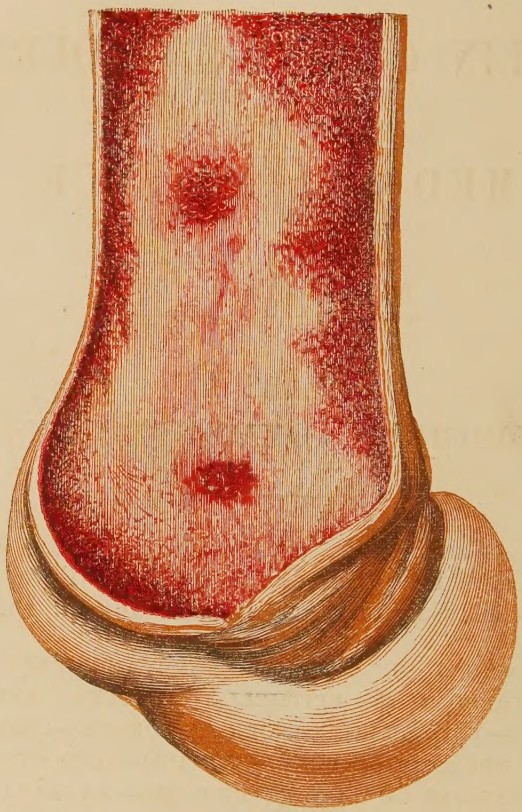

Fig. 7.

Osteitis engaging the lower part of the Femur of Robert S——.
The bone was very soft in the dark red portions of the section. Blood was extravasated in these situations.